Agricultural Research Institute

PUSA

PHILOSOPHICAL TRANSACTIONS,

GIVING SOME

ACCOUNT

OF THE

Present Undertakings, Studies, *and* Labours,

OF THE

INGENIOUS,

IN MANY

Confiderable Parts of the WORLD.

VOL. LVIII. For the Year 1768.

LONDON:

Printed for L. DAVIS and C. REYMERS,
Printers to the ROYAL SOCIETY,
againft *Gray's-Inn Gate*, in *Holbourn*.

M.DCC.LXIX.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume. And this information was thought the more necessary, not only as it has been the common opinion, that they were published by the authority, and under the direction, of the Society itself; but also, because several authors, both at home and abroad, have in their writings called them the *Transactions of the Royal Society*. Whereas in truth the Society, as a body, never did interest themselves any further in their publication, than by occasionally recommending the revival of them to some of their secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviseable, that a Committee of their Members should be appointed to reconsider the papers read before them, and select out of them such, as they should

ſhould judge moſt proper for publication in the future *Tranſactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance or ſingularity of the ſubjects, or the advantageous manner of treating them; without pretending to anſwer for the certainty of the facts, or propriety of the reaſonings, contained in the ſeveral papers ſo publiſhed, which muſt ſtill reſt on the credit or judgment of their reſpective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of nature or art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons, through whoſe hands they receive them, are to be conſidered in no other light, than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

CON-

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PHILO-

PHILOSOPHICAL TRANSACTIONS.

Received January 24, 1768.

I. *An Account of the Eruption of* Mount Vesuvius, *in* 1767: *In a Letter to the Earl of* Morton, *President of the Royal Society, from the Honourable* William Hamilton, *His Majesty's Envoy Extraordinary at* Naples.

Naples, December 29, 1767.

My Lord,

Read Feb. 11, 1768.

THE favourable reception, which my account of last year's eruption of Mount Vesuvius met with from your Lordship, the approbation which the Royal Society was pleased to shew, by having ordered the same to be printed in

their

their Philosophical Transactions, and your Lordship's commands in your letter of the 3d instant, encourage me to trouble you with a plain narrative of what came immediately under my observation during the late violent eruption, which began October 19, 1767, and is reckoned to be the 27th since that, which, in the time of Titus, destroyed Herculaneum and Pompeii.

The eruption of 1766 continued in some degree till the 10th of December, about nine months in all, yet in that space of time the moutain did not cast up a third of the quantity of lava, which it disgorged in only seven days, the term of this last eruption. On the 15th of December, last year, within the ancient crater of Mount Vesuvius, and about twenty feet deep, there was a crust, which formed a plain, not unlike the solfaterra in miniature; in the midst of this plain was a little mountain, whose top did not rise so high as the rim of the ancient crater. I went into this plain, and up the little mountain, which was perforated, and served as the principal chimney to the volcano; when I threw down large stones, I could hear that they met with many obstructions in their way, and could count a hundred moderately before they reached the bottom.

Vesuvius was quiet till March 1767, when it began to throw up stones, from time to time; in April the throws were more frequent, and at night fire was visible on the top of the mountain; or, more properly speaking, the smoak, which hung over the crater, was tinged by the reflection of the fire within the volcano. These repeated throws of cinders, ashes, and pumice stones, encreased the little

 mountain

mountain ſo much, that in May its top was viſible above the rim of the ancient crater. The 7th of Auguſt there iſſued a ſmall ſtream of lava, from a breach in the ſide of this little mountain, which gradually filled the valley between it and the ancient crater; ſo that the 12th of September the lava overflowed the ancient crater, and took its courſe down the ſides of the great mountain; by this time, the throws were much more frequent, and the red hot ſtones went ſo high as to take up ten ſeconds in their fall. Padre Torre, a great obſerver of Mount Veſuvius, ſays they went up above 1000 feet.

The 15th of October, the height of the little mountain (formed in about eight months) was meaſured by Don Andrea Pigonati, a very ingenious young man in his Sicilian majeſty's ſervice, who aſſured me that its height was 185 French feet.

From my villa, ſituated between Herculaneum and Pompeii, near the convent of the Calmaldoleſe (marked 7 in the incloſed Plan I.), I had watched the growing of this little mountain, and by taking drawings of it from time to time, I could perceive its increaſe moſt minutely; I make no doubt but that the whole of Mount Veſuvius has been formed in the ſame manner; and as theſe obſervations ſeem to me to account for the various irregular ſtrata, which are met with in the neighbourhood of volcanos, I have ventured to encloſe for your Lordſhip's inſpection a copy of the abovementioned drawings.

The lava continued to run over the ancient crater in ſmall ſtreams, ſometimes on one ſide, and ſome times on another, till the 18th of October, when I took particular notice that there was not the

leaſt lava to be ſeen, owing, I imagine, to its being employed in forcing its way towards the place where it burſt out the following day. As I had, contrary to the opinion of moſt people here, foretold the approaching eruption [a], and had obſerved a great fermentation in the mountain after the heavy rains, which fell the 13th and 14th of October, I was not ſurpriſed on the 19th following, at ſeven of the clock in the morning, to perceive from my villa every ſymptom of the eruption being juſt at hand. From the top of the little mountain iſſued a thick black ſmoak, ſo thick that it ſeemed to have difficulty in forcing its way out; cloud after cloud mounted with a haſty ſpiral motion, and every minute a volley of great ſtones were ſhot up to an immenſe height in the midſt of theſe clouds; by degrees, the ſmoak took the exact ſhape of a huge pine tree, ſuch as Pliny the younger deſcribed in his letter to Tacitus, where he gives an account of the fatal eruption in which his uncle periſhed [b]. This column of black ſmoak,

[a] This plainly appears from the following extract of a letter, from the ſame gentleman to the preſident, dated Naples, October 6, 1767. "Mount Veſuvius is preparing for another "eruption, or rather a ſecond part of the laſt, as it has never "been quiet ſince the beginning of the year 1765. The lava "already runs over the crater; and by the quantity of ſtones "and aſhes, the montagnola has almoſt filled the crater, and "has riſen at leaſt 80 feet within theſe laſt three months."

[b] Theſe are his words. "Nubes (incertum procul intuentibus ex quo monte Veſuvium fuiſſe poſtea cognitum eſt) oriebatur, cujus ſimilitudinem & formam, non alia magis arbor, "quam pinus expreſſerit. Nam longiſſimo veluti trunco elata "in altum, quibuſdam ramis diffundebatur, credo quia recenti "ſpiritu evecta, dein ſeneſcente eo deſtituta, aut etiam pondere "ſuo victa, in latitudinem evaneſcebat: candida interdum, in-"terdum ſordida & maculoſa, prout terram cineremve ſuſtu-"lerat." Plin. Lib. VI, Ep. 16.

after

after having mounted an extraordinary height, bent with the wind towards Caprea, and actually reached over that iſland, which is not leſs than 28 miles from Veſuvius.

I warned my family not to be alarmed, as I expected there would be an earthquake at the moment of the lava's burſting out; but before eight of the clock in the morning I perceived that the mountain had opened a mouth, without noiſe, about 100 yards lower than the ancient crater, on the ſide towards the Monte di Somma; and I plainly perceived, by a white ſmoak, which always accompanies the lava, that it had forced its way out: as ſoon as it had vent, the ſmoak no longer came out with that violence from the top. As I imagined that there would be no danger in approaching the mountain when the lava had vent, I went up immediately, accompanied by one peaſant only. I paſſed the hermitage (3. in Plan I.), and proceeded as far as the ſpot marked (×), in the valley between the mountain of Somma and that of Veſuvius, which is called Atrio di Cavallo. I was making my obſervations upon the lava, which had already, from the ſpot (E) where it firſt broke out, reached the valley, when, on a ſudden, about noon, I heard a violent noiſe within the mountain, and at the ſpot (C) about a quarter of a mile off the place where I ſtood, the mountain ſplit; and, with much noiſe, from this new mouth a fountain of liquid fire ſhot up many feet high, and then like a torrent rolled on directly towards us. The earth ſhook at the ſame time, that a volley of pumice ſtones fell thick upon us; in an inſtant, clouds of black ſmoak and aſhes cauſed almoſt a total darkneſs; the exploſions

ſions from the top of the mountain were much louder than any thunder I ever heard, and the ſmell of the ſulphur was very offenſive. My guide alarmed took to his heels; and I muſt confeſs that I was not at my eaſe. I followed cloſe, and we ran near three miles without ſtopping; as the earth continued to ſhake under our feet, I was apprehenſive of the opening of a freſh mouth, which might have cut off our retreat. I alſo feared that the violent exploſions would detach ſome of the rocks off the mountain of Somma, under which we were obliged to paſs; beſides, the pumice-ſtones, falling upon us like hail, were of ſuch a ſize as to cauſe a diſagreeable ſenſation upon the part where they fell. After having taken breath, as the earth ſtill trembled greatly, I thought it moſt prudent to leave the mountain, and return to my villa, where I found my family in a great alarm at the continual and violent exploſions of the volcano, which ſhook our houſe to its very foundation, the doors and windows ſwinging upon their hinges. About two of the clock in the afternoon another lava forced its way out of the ſame place from whence came the lava laſt year, at the ſpot marked B (in Plan II.), ſo that the conflagration was ſoon as great on this ſide of the mountain as on the other, which I had juſt left.

The noiſe and ſmell of ſulphur encreaſing, we removed from our villa to Naples; and I thought proper, as I paſſed by Portici, to inform the court of what I had ſeen; and humbly offered it as my opinion, that his Sicilian majeſty ſhould leave the neighbourhood of the threatening mountain. However, the court did not leave Portici till about twelve [illegible] lock, when the lava had reached as far as

(4. in

(4. in Plan. I.) I obſerved, in my way to Naples, which was in leſs than two hours after I had left the mountain, that the lava had actually covered three miles of the very road through which we had retreated. It is aſtoniſhing that it ſhould have run ſo faſt; as I have ſince ſeen, that the river of lava, in the Atrio di Cavallo, was 60 and 70 feet deep, and in ſome places near two miles broad. When his Sicilian majeſty quitted Portici, the noiſe was greatly increaſed, and the confuſion of the air from the exploſions was ſo violent, that, in the king's palace, doors and windows were forced open, and even one door there, which was locked, was nevertheleſs burſt open. At Naples, the ſame night, many windows and doors flew open; in my houſe, which is not on the ſide of the town next Veſuvius, I tried the experiment of unbolting my windows, when they flew wide open upon every exploſion of the mountain. Beſides theſe exploſions, which were very frequent, there was a continued ſubterraneous and violent rumbling noiſe, which laſted this night about five hours. I have imagined that this extraordinary noiſe might be owing to the lava in the bowels of the mountain having met with a depoſition of rain water, and that the conflict between the fire and the water may, in ſome meaſure, account for ſo extraordinary a crackling and hiſſing noiſe. Padre Torre, who has wrote ſo much and ſo well upon the ſubject of Mount Veſuvius, is alſo of my opinion; and indeed it is natural to imagine, that there may be rain water lodged in many of the caverns of the mountain, as, in the great eruption of Mount Veſuvius in 1663, it is well atteſted, that ſeveral towns,

among

among which Portici and Torre del Greco, were deſtroyed by a torrent of boiling water having burſt out of the mountain with the *lava*, by which thouſands of lives were loſt. About four years ago, Mount Etna in Sicily threw up hot water alſo, during an eruption.

The confuſion at Naples this night cannot be deſcribed; his Sicilian majeſty's haſty retreat from Portici added to the alarm; all the churches were opened and filled, the ſtreets were thronged with proceſſions of ſaints; but I ſhall avoid entering upon a deſcription of the various ceremonies that were performed in this capital, to quell the fury of the turbulent mountain.

Tueſday the 20th, it was impoſſible to judge of the ſituation of Veſuvius, on account of the ſmoak and aſhes which covered it entirely, and ſpread over Naples alſo, the ſun appearing as through a thick London fog, or a ſmoaked glaſs; ſmall aſhes fell all this day at Naples. The lavas on both ſides of the mountain ran violently; but there was little or no noiſe till about nine o'clock at night, when the ſame uncommon rumbling began again, accompanied with exploſions as before, which laſted about four hours; it ſeemed as if the mountain would ſplit in pieces; and, indeed, it opened this night almoſt from the ſpot E to C (in Plan I.). The incloſed plans were taken upon the ſpot at this time, when the lava's were at their height; and I do not think them exaggerated. The Pariſian barometer was, as yeſterday, at 27. 9, and Fahrenheit's thermometer at 70 degrees; whereas, for ſome days preceding the eruption, it had beeen at 65 and 66. During the confuſion

confusion of this night the prisoners in the publick jail attempted to escape, having wounded the jailer, but were prevented by the troops. The mob also set fire to the cardinal archbishop's gate, because he refused to bring out the relicks of Saint Januarius.

Wednesday 21st was more quiet than the preceding days, though the lavas ran briskly. Portici was once in some danger, had not the lava taken a different course, when it was only a mile and a half from it; towards night the lava slackened.

Thursday 22d, about ten of the clock in the morning, the same thundering noise began again, but with more violence than the preceding days; the oldest men declared they had never heard the like, and, indeed, it was very alarming; we were in expectation every moment of some dire calamity. The ashes, or rather small cinders, showered down so fast, that the people in the streets were obliged to use umbrellas, or flap their hats, these ashes being very offensive to the eyes. The tops of the houses, and the balconies, were covered above an inch thick with these cinders. Ships at sea, twenty leagues from Naples, were also covered with them, to the great astonishment of the sailors. In the midst of these horrors, the mob growing tumultuous and impatient, obliged the cardinal to bring out the head of Saint Januarius, and go with it in procession to the Ponte Maddalena, at the extremity of Naples, towards Vesuvius; and it is well attested here, that the eruption ceased the moment the Saint came in sight of the mountain; it is true the noise ceased about that time, after having lasted five hours, as it had done the preceding days.

Friday 23d, the lavas ſtill ran, and the mountain continued to throw up quantities of ſtones from its crater; there was no noiſe heard at Naples this day, and but little aſhes fell there.

Saturday 24th, the lava ceaſed running; the extent of the lava, from the ſpot C (Plan I.), where I ſaw it break out, to its extremity F, where it ſurrounded the chapel of Saint Vito, is above ſix miles. In the Atrio di Cavallo, and in a deep valley, that lies between Veſuvius (1.), and the hermitage (3.), the lava is in ſome places near two miles broad, and in moſt places from 60 to 70 feet deep; at (4.) the lava ran down a hollow way, called Foſſa grande, made by the currents of rain water; it is not leſs than 200 feet deep, and 100 broad; yet the lava in one place has filled it up. I could not have believed that ſo great a quantity of matter could have been thrown out in ſo ſhort a time, if I had not ſince examined the whole courſe of the lava myſelf. This great compact body will certainly retain ſome heat many months; at this time, much rain having fallen for ſome days paſt, the lava ſmoaks, as if it ran a-freſh: and about ten days ago, when I was up the mountain with Lord Stormont, we thruſt ſticks into the crevices of the lava, which took fire immediately: But to proceed with my journal.

The 24th Veſuvius continued to throw up ſtones as on the preceding days; during the whole of this eruption it had differed in this circumſtance from the eruption of 1766, when no ſtones were thrown out of the crater from the moment the lava ran freely.

Sunday 25th, ſmall aſhes fell all day at Naples; they iſſued from the crater of the volcano, and formed

a vaſt column, as black as the mountain itſelf, ſo that the ſhadow of it was marked out on the ſurface of the ſea; continual flaſhes of forked, or zig-zag lightning ſhot from this black column, the thunder of which was heard in the neighbourhood of the mountain, but not at Naples; there were no clouds in the ſky at this time, except thoſe of ſmoak iſſuing from the crater of Veſuvius. I was much pleaſed with this phænomenon, which I had not ſeen before in that perfection.

Monday 26th, the ſmoak continued, but not ſo thick, neither were there any flaſhes of the mountain lightning. As no lava has appeared after this column of black ſmoak, which muſt have been occaſioned by ſome inward operation of fire, I am apt to think that the lava, which ſhould naturally have followed this ſymptom, has broke its way into ſome deeper cavern, where it is ſilently brooding future miſchief; and I ſhall be much miſtaken if it does not break out a few months hence.

Tueſday 27th, no more black ſmoak, nor any ſigns of eruption.

Thus, My Lord, I have had the honor of giving your Lordſhip a faithful narrative of my obſervations during this eruption, which is univerſally allowed to have been the moſt violent of this century; and I ſhall be happy if it ſhould meet with your approbation, and that of the Royal Society, if your Lordſhip ſhould think it worthy of being communicated to ſo reſpectable a body.

I have juſt ſent a preſent to the Britiſh Muſeum of a compleat collection of every ſort of matter produced by Mount Veſuvius, which I have been col-

lecting with ſome pains for theſe three years paſt, and it will be a great ſatisfaction to me if, by the means of this collection, ſome of my countrymen, learned in natural hiſtory, may be enabled to make ſome uſeful diſcoveries relative to volcanos [c].

I have alſo accompanied that collection with a current of lava from Mount Veſuvius; it is painted with tranſparent colours, and, when lighted up with lamps behind it, gives a much better idea of Veſuvius, than is poſſible to be given by any other ſort of painting.

I have the honor to be,

My Lord,

Your Lordſhip's

moſt obedient,

and moſt humble ſervant,

William Hamilton.

[c] "I am well convinced, by this collection, that many vari-"egated marbles, and many precious ſtones, are the produce "of volcanos; and that there have been volcanos in many parts "of the world, where at preſent there are no traces of them "viſible." This is taken from a prior letter of Mr. Hamilton, to the Preſident, dated April 7, 1767.

PLATE

PLATE I.

A. Crater of Mount Vesuvius.
B. Mouth from whence came the lava of 1766; and which opened afresh, October 19, 1767, and produced the conflagration represented in Plan II.
C. The mouth which opened at 12 o'clock, October 19, 1767, whilst I was at the spot marked ×; from thence came all the lava represented in Plan I.
D. The lava.
E. Mouth from whence the lava flowed at eight o'clock, October 19, when the eruption began first.
F. Chapel of Saint Vito surrounded with lava.

1. Vesuvius.
2. Mountain of Somma.
3. Hermitage, between which and Vesuvius there is a deep valley two miles broad.
4. The Fossa Grande.
5. His Sicilian Majesty's Palace at Portici.
6. Church of Pugliano.
7. Calmaldolese Convent, near which is my Villa.
8. Saint Torio.
9. Barra.
10. Spot, under which lies Herculaneum.

PLATE II.

A. Crater of Vefuvius.

B. Mouth, from whence came the lava of 1766, and which opened afrefh at two o'clock, October 19, 1767, and caufed the conflagration on this fide of the mountain.

C. Mouth which opened at 12 o'clock, October 19, 1767, whilft I was at the fpot X, and which produced all the lava reprefented in Plan I.

D. Rivulets of lava, which flowed from the crater, and united with the great river E.

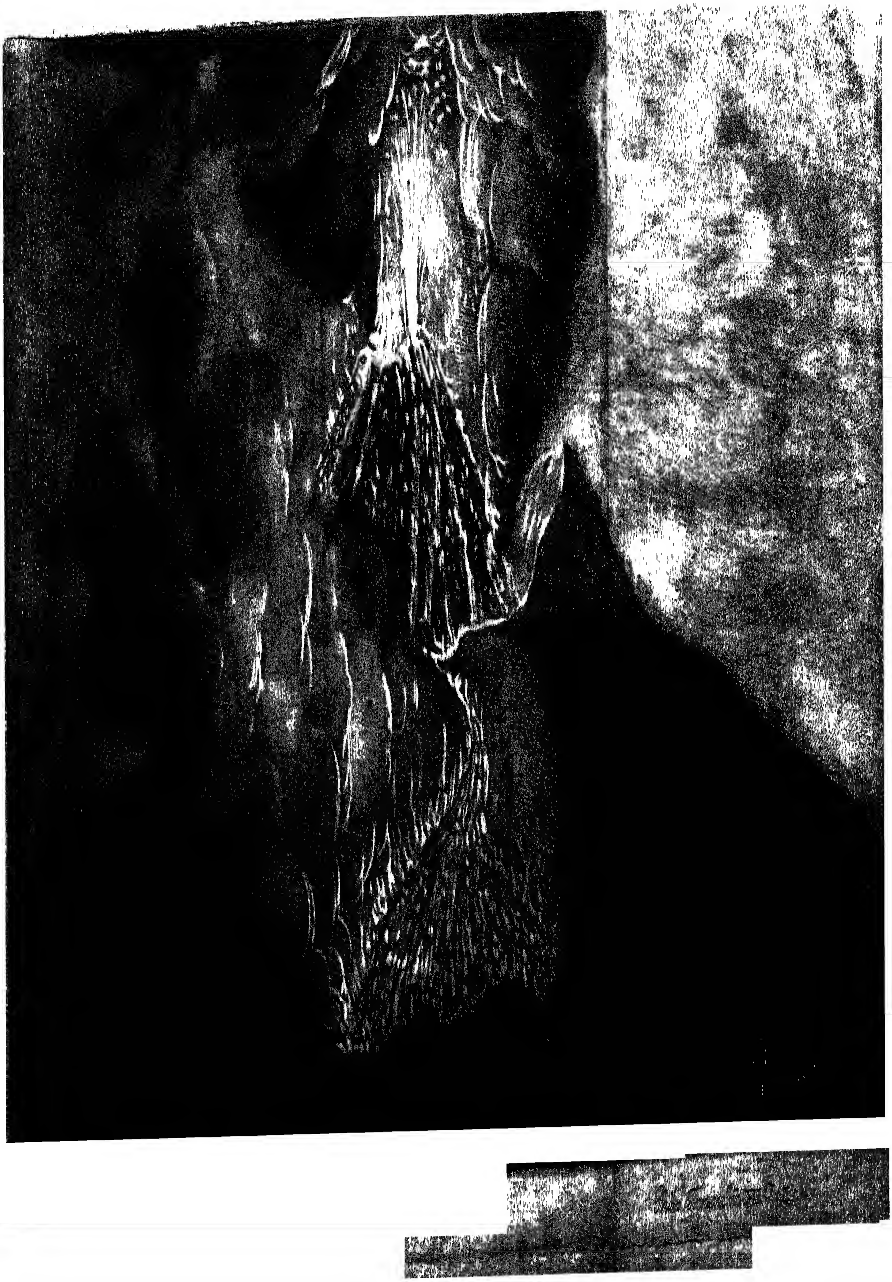
F. Extremities of the lava, about five miles from B.

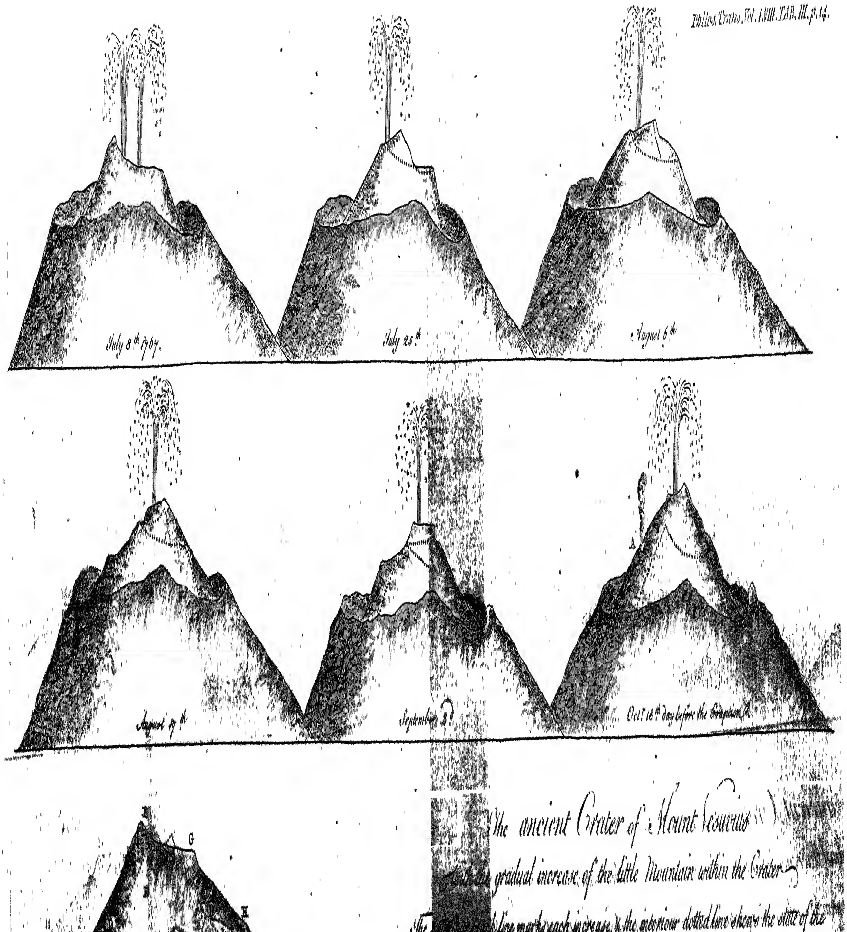
1. Mountain of Somma.
2. Mount Vefuvius.
3. Montagna di Trecafe.
4. Trecafe.
5. Oratorio di Bofco.
6. Ottaiano.

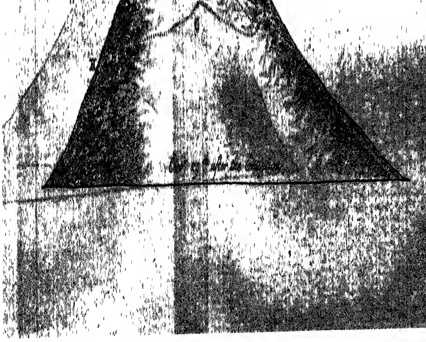
PLATE III.

Views of the gradual increafe of the little mountain within the ancient crater, and of the prefent ftate of Mount Vefuvius.

[redacted]







The ancient Crater of Mount Vesuvius

with the gradual increase of the little Mountain within the Crater

The [illegible] line marks each increase, & the interiour dotted line shews the state of the little [illegible] that increase, so that the dotted line in the Drawing of Octr 18th shews the [illegible]

[illegible]

[illegible] which is near 200 feet [illegible]

Received November 26, 1767.

II. *Extract of a Letter, dated* Vienna April 4, 1767, *from Father* Joseph Liesganig, *Jesuit, to Dr.* Bevis, *F. R. S. containing a short Account of the Measurement of Three Degrees of Latitude under the Meridian of* Vienna.

Read Feb. 18, 1767.

POST paucas septimanas prelo committam opusculum meum de dimensione trium fere graduum meridiani nostri, Augustæ nostræ jussu a me suscepta. Continetur arcus totus quem dimensus sum, inter pagum Sobieschiz (Brunnâ, Moraviæ urbe, 3′½ borealiorem) ac inter Varasdinum Croatiæ. Arcum terrestrem definivi 22 triangulis sat magnis, si ea demam, quæ duas bases (quarum quævis 6000 hexapedarum Parisinarum superat) cum triangulorum serie connectunt. Arcûs cœlestis amplitudinem determinavi per observationes plurium fixarum, quas feci sectore decem pedum, non in extremis duntaxat arcûs mei, locis Sobieschizii videlicet et Varasdini, verum etiam Brunnæ, Viennæ, et Græcii, Styriæ metropoli: quo magnitudinem unius gradûs, tum ex arcu toto, tum ex interjectis partialibus arcubus deducere liceret. Nec imprudenter id a me actum esse eventus docuit: nam gradum inter Viennam et Græcium interceptum 186 hexapedas Parisinas minorem reperi eo, qui Viennâ Brunnam, Boream versus porrigitur, Varasdinensem fere 300 hexapedis Brunnensi majorem. Insignem hanc differentiam attractioni ingentium Styriæ superioris ac inferioris montium, quibus Græcium subjicitur, tribuendam esse ostendam. Adjecta pagina compendio totius laboris ac opusculi mei summam exhibet.

cus Meridiani Cœleſtis.

	Dracon	γ Dracon.	Cycni.	α Cycni.	Capra.	β Aurigæ.	Medium.	
	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″	
Inter Sobieſchiz, et Brunnam		0 3 35,8					0 3 35 8	1
et Viennam		1 2 29,0				1 2 27,8	1 2 29,0	2
et Græcium		2 10 54 3				2 10 55 8	2 10 55,0	3
et Varaſdin.						2 56 45,7		
Inter Brunnam et Viennam	0 58 53,2	0 58 53,6					0 58 53,5	4
et Græcium	2 7 18,2	2 7 18,4					2 7 18,3	5
Inter Viennam et Græcium	1 8 24,9	1 8 24,7				1 8 28,0	1 8 25,8	6
						vel	1 8 24,8	7
et Varaſdin.						1 54 17,9	1 54 17,9	8
Inter Græcium et Varaſdin.			0 45 50,5	0 45 48,9	0 45 50,1	1 45 49,9	0 45 49,9	9

	Amplit. arcus cœleſtis.	Diſtantia parallelorum.	Ex ſerie triangulorum.	Magnitudo unius Gradus. Hex. Vienn.	Hex. Pariſ.	Red. ad libell. maris.
	° ′ ″					
Inter Sobieſchiz et Varaſdin.	2 56 45,8	172808,1		58657,5	57079,8	57077,4
Inter Sobieſchiz et Viennam	1 2 29,0	61094,0	I.	58665,9	57088,0	57085,6
		61090,7	II.	58662,7	57084,9	57082,5
		61088,8	III.	58660,9	57083,1	57080,7
Inter Brunnam et Viennam	0 58 53,5	57588,5		58672,3	57094,2	57091,8
			Medium	58665,4	57087,5	57085,1
Inter Viennam et Græcium	1 8 25,8	66681,8		58467,2	56894 6	56892,2
vel	1 8 24,8			58481,4	56908,5	56906,1
			Medium	58474,3	56901,5	56899,1
Inter Græcium et Varaſdin.	0 45 49,9	45032,3		58953,5	57367,9	57365,5

Received

Received November 16, 1767.

III. *An Essay on the Force of Percussion, by* William Richardson, *M. D. communicated by* William Heberden, *M. D. F. R. S.*

Read Feb. 18, 1768.

WHEN we consider the extraordinary advancement in natural philosophy, from the surprising discoveries of the great Sir Isaac Newton, and other ingenious men, who have followed his example; it may afford matter of the greatest wonder, to find the most acute philosophers still contending, whether the force of percussion be in proportion to the velocity of bodies in motion, or the squares of those velocities.

Sentiments so opposite in their nature, and so strongly supported by their respective advocates, may well make a cautious person suspect, that nothing is to be discovered with certainty in the operations of nature. In which opinion he may be the more confirmed from this consideration, that the present dispute is not about objects far removed from our observation, but such actions of bodies as do constantly occur to our senses, and which without difficulty may be reduced to experiment.

What then can be the reason, after such variety of experiments have been made, why this matter is not, before this time, brought to a decision? The

 fault

fault cannot be in the experiments themselves, the chief of which have been often repeated, and that in the most accurate manner: it must, therefore, be in the use which has been made of them, in the groundless inferences which have been drawn from them.

Those who have wrote best on this subject (whether in support of the velocities, or the squares of velocities) seem to me to have inferred more from the principle they maintain than what they bring sufficient arguments to justify; by which means they blend truth with error, and the more they endeavour to illustrate their respective doctrines, the more they render them perplexed and confused. For the truth of what I have advanced, I appeal to the two following instances.

Those who maintain that the force of percussion is as the velocity of the striking bodies, when they account for the impressions made in soft bodies (which are found, by experiment, to be as the squares of the velocities), inform us, that the time ought to be taken into the account; which being as the velocity of the impinging body, the impression will of course be as the time into the velocity, or (which is the same thing) as the square of the velocity.

But this, in my mind, is to assert more than what can be clearly demonstrated: for as action and reaction are equal, the more forcibly one body acts upon another, the greater must be the resistance it meets with; and whatever be the time of its acting, supposing its force to be given, the effect produced must still remain the same.

To which I muſt further add, that ſome of the moſt learned and zealous advocates for time being taken into the account, have not agreed among themſelves whether it be in a direct or reciprocal proportion of the velocity[a].

Thoſe who, on the contrary, inſiſt that the force of percuſſion is in proportion to the ſquares of the velocity, finding from experiment that in ſoft bodies the velocity after percuſſion falls ſhort of this eſtimate, would make us believe, that in compreſſing the parts of thoſe bodies, a certain degree of force muſt neceſſarily be loſt, which, being added to what remains after percuſſion, will ſufficiently confirm the truth of their doctrine.

To this I reply, that the parts of ſoft bodies are, indeed, removed out of their places by the ſtroke, and that ſome motion is loſt in the impinging body, being communicated to the parts of the ſoft body it ſtrikes upon; but theſe parts cannot loſe their motion any other way, than by communicating it to other parts, or by the force accruing to the whole body.

How then are theſe different effects to be accounted for, and in what manner are they to be deduced from the ſame cauſe? This diverſity of appearances, I have for ſome time ſuſpected, might proceed from the nature of coheſion: that while the force of percuſſion produced an effect on the whole maſs of matter which receives the ſtroke, in proportion to the velocity of the impinging body; it might, at the

[a] Dr. Pemberton, Philoſophical Tranſactions, N° 371. p. 57. Dr. Clarke, Philoſophical Tranſactions, N° 401. p. 382.

ſame time, in ſeparating the cohering parts from each other, produce an effect in proportion to the ſquare of the velocity.

Into which way of thinking I was firſt led from the following obſervations; that a chord, which would bear a very ſtrong pull, might eaſily be broken by giving it a ſudden jerk; as alſo that the weight of a hammer did not contribute ſo much in driving a nail, as the quickneſs of the motion given it by the driver.

In order to make a further diſcovery, whether or no this my ſuppoſition was really founded in nature; I determined firſt to make experiments on ſuch ſoft bodies as have a conſiderable degree of coheſion; and then to try thoſe bodies, when dried and reduced to powder, and by that means deprived of their coheſion; which experiments, when compared with each other, would, I flattered myſelf, give me an inſight into this intricate affair, and at the ſame time diſcloſe that beautiful ſimplicity, which nature obſerves in all her operations.

My apparatus for making the experiments conſiſts of four balls exactly ſpherical, two iron branches, and a ſmall lead ciſtern.

The balls are each of them two inches in diameter; two are of braſs, and two of box-wood; one of each ſort is ſolid, and the other hollow; that which is hollow is only half the weight of the ſolid one, and may be opened by means of a ſcrew in the middle.

The iron branches are to give the balls their proper directions; they have each of them a ſmall braſs pully in the fore part, and in the hind part a kind of

hook,

hook, which faſtens them to ſtaples at different heights; one of the branches is two inches long, and the other four inches, excluſive of pullies; by which means the balls when let fall are directed to different parts of the ſurface they ſtrike upon.

The lead ciſtern is of an oblong form, that the matter therein contained may, at the ſame time, receive two diſtinct impreſſions; either when balls of different weight are let fall, or the ſame ball is let fall from different heights; its length is ſix inches, its breadth four inches, and its depth two inches.

The matter I have found beſt ſuited to the purpoſe is ſtiff clay, tempered in ſuch a manner as to be ſmooth and uniform, with the ſame reduced to powder, after having been baked in an oven, as alſo after having (by a ſtill ſtronger heat) been converted into brick.

The ciſtern is by turns filled with theſe different materials, which are to be cloſely and uniformly preſſed down, ſo as to leave the ſurface quite level: in effecting which, great caution is required, more particularly in regard to the powders; as they will not diſtinctly retain the impreſſions, unleſs they have ſome ſmall degree of moiſture, or be very cloſely preſſed down; in both which caſes they acquire ſuch a degree of coheſion, as of courſe muſt render the experiments more or leſs imperfect.

Things being thus prepared, in order to try the neceſſary experiments, I fixt the ſtaples, and by their means the branches, at the following heights, viz. two feet, four feet, and eight feet; the reſult of which experiments was as follows.

When

When the braſs balls, in weight to each other as two to one, were let fall on tempered clay, from four feet and eight feet reſpectively, the impreſſions made were on various trials found to be equal.

When the wood balls, being to each other as two to one, were let fall from the ſame heights, on dried clay pulveriſed, that from four feet generally made the deeper impreſſion.

When the wood balls were let fall from the ſame heights on brick-duſt, that from four feet conſtantly made the deeper impreſſion.

When the lighter braſs ball was let fall on tempered clay, from two feet and eight feet, the impreſſions were to each other as one to four.

When the lighter wood ball was let fall on dried clay pulveriſed, from the ſame heights, the impreſſions were (ſo far as the eye could judge) nearly in the proportion of one to three.

When the ſame ball was let fall on brick-duſt, from the like heights, the impreſſions were not much ſhort of the proportion of one to two.

From theſe experiments it plainly appears: Firſt, That the impreſſions made in ſoft clay are in proportion to the heights, from whence the balls are let fall, conſequently as the ſquares of their reſpective velocities. Secondly, That the impreſſions, in pulveriſed clay, recede conſiderably from that proportion, being as it were in the medium between the ſquares of the velocities and the velocities themſelves. Thirdly, That the impreſſions in brick-duſt are nearly in a ſubduplicate proportion of the heights from whence the balls are let fall, conſequently vary

but

but little from the proportion of the velocities acquired.

Whence I ſhould apprehend it clearly follows; that the impreſſions made in ſoft bodies, by hard ones ſtriking upon them, do vary from each other, according to the degree of coheſion in the reſpective ſoft bodies; and that the impreſſions would be in exact proportion of the velocities, could their form be perfectly retained by bodies quite void of coheſion.

Nothing, however, being more evident to me than that actions ought to be meaſured by their effects, and at the ſame time fully depending on the accuracy of the experiments, I am determined to reſt this important point entirely upon them. Shall not, therefore, attempt any illuſtration in the mathematical way, leſt, by too far indulging a favourite opinion, I ſhould bewilder myſelf in intricate calculations. Much leſs ſhall I endeavour to eſtabliſh my doctrine on metaphyſical principles, which ſeem to me in themſelves too obſcure, to throw any clear light on ſubjects of this nature.

Received November 26, 1767.

IV. *An Essay on the Connexion between the Parallaxes of the Sun and Moon; their Densities; and their disturbing Forces on the Ocean. By* Patrick Murdoch, *D. D. F. R. S.*

Read Feb. 18, 1768.

IN a letter to Mr. Reid, formerly presented to the Hon. Society, and printed in Vol. LIV. Part II. of the Transactions, mention was made of a rule which I had used for computing the sun's parallax; but as that rule, though it gave a solution near the truth, was in part founded on authority (which, however respectable, ought to be cautiously admitted in such enquiries), I have considered the subject anew, upon such principles alone, as the established theory and the best observations furnished me. And the result is now humbly submitted to the Society.

I. The length of a second-pendulum at the equator, and on a level with the sea, being 36 inches $7\frac{1}{10}$ lines, Paris measure, according to the accurate observations of M. de la Condamine *, that length (properly

* Journal du Voyage, &c. p. 163. In the copy of this paper, which was read in the Society, the length of a second-pendulum, at the equator, had been computed from its length at London: but here it is taken from the immediate observations of a very able astronomer. Some other small alterations have been made; and the examples placed in a better order.

(properly corrected) will, by the reasoning in the abovementioned letter, give the distance of a moon circulating round an unmoveable earth, equal to 59.95792 semidiameters of the equator. For the logarithm of this number, which is 1.7778438, write l.

Let L be the logarithm of some greater mean distance, inferred from observations of the moon's parallax; and if r be the natural number to the logarithm $3 \times \overline{L - l}$, and M be taken equal to $\frac{1}{r-1}$, the mass of the earth will be to that of the moon, as M to 1.

Conversely, If M be any how determined, its equal $\frac{1}{r-1}$, and r, with its logarithm $3 \times \overline{L - l}$ are known; $\frac{1}{3}$ of which is $\overline{L - l}$ to be added to l. For instance, if, with Sir Isaac Newton, we put $M = 39.788$, the distance will be 60.4557, the logarithm L being 1.7814372.

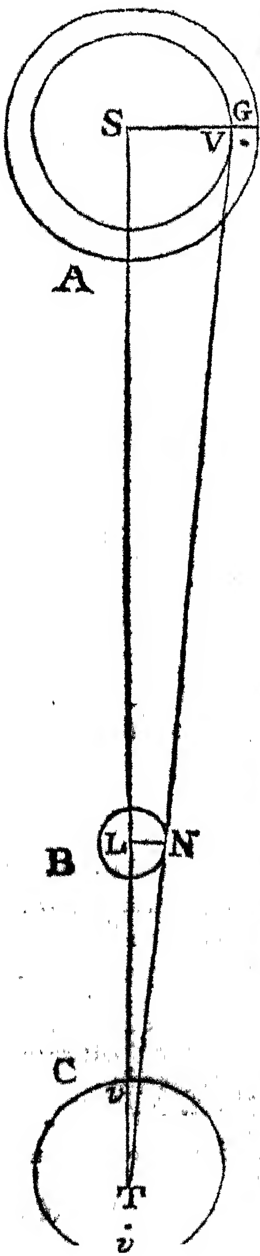
II. If, for each of these three, the moon's mass, her accelerative force on the earth, and her distance from the earth's centre, we write ($\varphi =$) 1: the accelerative force of the earth on the moon will be represented by M, the mass just now computed. And if F is the sun's accelerative force on the earth, x his distance in semidiameters of the lunar orbit, Q the ratio of a sidereal year to a periodic month; we have (by Cor. 2. Prop. 4. Princip. I.) $\frac{F}{x} = \frac{M}{Q^2}$; a given ratio in given terms.

III. The terms F, x, therefore, muſt involve a common factor; by which being divided, the quote may be $\frac{M}{Q^2}$. And this might be obtained innumerable ways, were we to conſider the ratio $\frac{F}{x}$ merely as an abſtract quantity, altogether unreſtricted: it were only putting $M^n \times Q^p = F$. And $\frac{Q^{2+p}}{M^{1-n}} = x$, or $M^{1-n}Q^p = F$, and $\frac{Q^{2+p}}{M^n} = x$: ſo as the ſum of the *indices* of M ſhould be unity, and the difference of thoſe of Q ſhould be 2.

But though the quantities F, x, are as yet unknown, they are not for that indeterminate and variable, as ſuch a liberty of ſubſtitution would import: and all ſubſtitutions which imply the contrary, all *indices* which the theory diſowns, or which are inconſiſtent with obſervation, are to be rejected. In a word, the *indices*, n, p, ought each of them to be *unique*, and determinate *(ſine compare)*, * as the quantities F and x are in nature. I take, therefore, $p = 1$, and $n = \frac{1}{2}$; that is, $F = M^{\frac{1}{2}} \times Q$, $x = \frac{Q^3}{M^{\frac{1}{2}}}$. See the examples in the table ſubjoined, upon different ſuppoſitions of the moon's diſtance.

* See Neut. Arith. Univerſal. in the Schol. to Prob. xxiv. The *maxima* and *minima* of variable quantities; the coordinates belonging to a double point, or to a point of reflexion, or contrary flexure, rays of curvature, limits of ratios, &c. All theſe are examples of the *unique*; that is, of quantities in a ſtate that is diſtinguiſhed from and excluſive of all others.

IV. The



IV. The accelerative forces of two ſpherical bodies, A, B, upon a third C, are directly as their maſſes, and inverſely as the ſquares of their central diſtances S T, L T, (or of x and d): which may be thus expreſſed, $\frac{F}{\varphi} = \frac{A}{B} \times \frac{d^2}{x^2}$.

But the maſſes, A, B, being as their reſpective denſities (which call s, m), and the cubes of (R, r), the ſemidiameters of the ſpheres conjunctly; if we write for $\frac{A}{B}$ its equal, we have $\frac{F}{\varphi} = \frac{s}{m} \times \frac{R^3}{r^3} \times \frac{d^2}{x^2}$.

Let the maſs C be to B, as M to 1; and (f) its force on B, will be φ M, or $\varphi = \frac{f}{M}$, which gives $\frac{MF}{f} = \frac{s}{m} \times \frac{x}{d}$, and $\frac{F}{f} = \frac{sx}{Mmd}$: ſuppoſing the apparent ſemidiameters of A and B to ſubtend the ſame angle at the centre of C, and thence R to be to r, as x to d.

V. But if S G, the ſemidiameter of A, be to the ſuppoſed ſemidiameter S V, as q to 1, then, the

the denſity s remaining, the accelerative force of A (proportional to its magnitude) will be increaſed in the triplicate of that ratio, that is, we now have $\frac{F}{f} = \frac{q^3 s}{M m} \times x$, putting $d = 1$.

And the three bodies, A, B, C, repreſenting the ſun, moon, and earth; likewiſe Q being the ratio of the periods of the earth and moon, it is $\frac{F}{f} = \frac{x}{Q^2}$ (by the corollary quoted in Art. II.). Whence $\frac{s}{m} = \frac{M}{Q^3 \times Q^2}$.

VI. This accelerative force of A remaining, imagine the ſemidiameter S G to be reduced to its former magnitude S V; and the denſity of A will, at the ſame time, be increaſed to $s^1 = q^3 \times s$, and $\frac{s^1}{m} = \frac{M}{Q^2}$.

In which caſe, namely, when the apparent ſemidiameters of A and B (the ſun and moon) are equal, their powers to raiſe a tide at v, a vertex of C, will be as the denſities s^1, m*: that is, as M the ratio of the earth's maſs to the moon's, and Q^2 the duplicate ratio of the year and month.

Or thus: The diſtances of the bodies, A, B, from the third C, being very great, their powers to raiſe a tide at v, or their diſturbing forces on the ocean, will be directly as their accelerative forces at the centre of C, and inverſely as their diſtances from it: that is, writing a, b, for the diſturbing forces reſpectively; and for the ſun's diſtance in ſemidiameters of C, the Letter z, it will be $a : b :: \frac{F}{z} : \frac{\phi}{d}$.

* See the laſt page of Dr. Sanderſon's Fluxions: or the late ingenious T. Simpſon's Miſcellaneous Tracts, p. 13.

For,

For, by the general law, a is to F, as the difference of the squares of z and $\overline{z-1}$ is to the square of $\overline{z-1}$; that is, as $2z-1$ to z^2-2z+1. And the same way b is to φ, as $2d-1$ to d^2-2d+1; whence, halving the antecedents, and retaining only z^2, d^2, in the consequents, we have the ratio of a to b, as above.

If the disturbing force of B is exerted at v^1, the opposite vertex of C, b will now be to φ, as $2d+1$ to d^2+2d+1; and, in strictness, we ought to take a mean value of b: but this may be neglected as inconsiderable.

Lastly, let the sun's distance be again expressed in semidiameters of the lunar orbit; that is, if for z we write dx, and unity for φ, we have $a:b::\frac{F}{dx}:\frac{1}{d^2}$, or as M to Q^2, as before.

VII. In Art. V. it was found that m denoting the density of the moon, and s that of the sun, q^3 being triple the ratio of the sun's mean semidiameter to the moon's *, then will $\frac{m}{s}=\frac{Q^2q^3}{M}$. Whence it will easily follow, that the density of the earth is to that of the sun, as $Q^2\times S^3$ to P^3; P being the moon's horizontal parallax, and S the sun's apparent semidiameter.

* In the *Principia*, the semidiameters are 16′. 6″, and 15′. 38¼″; giving 0.0379755 for the logarithm of q^3. Others take a few seconds from each; which does not much alter the value of q^3.

VIII. I

VIII. I have applied these rules (as in the following Table) to the principal hypothesis of the moon's mean distance.

1°. Supposing it of 60.24 (from Dr. M. Stewart.)
2°. ———————— 60.4 (Cor. 7. Prop. 37. Princip. III.)
3°. ———————— 60.455 (Neut. M being 39.788.)
4°. ———————— 60.493 (As by Mr. Short's calculations from the transit of ♀.)

The Moon's Mean Distance, being in Semidiameters of the Equator.

		60.24	60.4	60.455	60.493
I.	Parall. ☽	′ ″ 57.4,17	′ ″ 56.55 $\frac{1}{15}$	′ ″ 56.52	′ ″ 56.49,88
II.	Mass ♂/☽	70.4225	44.823	39.788	36.9908
III.	☉ dist. to ☽	284.723	356.885	378.293	392.854
IV.	Parall. ☉	″ 12 $\frac{1}{37}$	″ 9,58	″ ‴ 9. $\frac{1}{2}$	″ 8.69
V.	Dens. ☽/☉	2,77 : 1	4,35 : 1	4,9 : 1	5,273 : 1
VI.	Dens. ♂/☽	1,449 : 1	0.9295 : 1	0.827 : 1	0.7707 : 1
VII.	Dens. ♂/☉	4.0128 : 1	4.045 : 1	4.0559 : 1	4.06375 : 1
VIII.	Tides ☽/☉	2.5379 : 1	3.9873 : 1	4.5544 : 1	4.8316 : 1
IX.	F. ☉ on ♂ F. ♂ on ☽	1.593 : 1	1.9968 : 1	2.1194 : 1	2.1981 : 1

R. E.

REMARKS.

1. If it ſhould be thought that the reaſoning in Art. III. reſts too much upon a metaphyſical principle of Leibnitz, and requires, if not an apology, at leaſt a more formal proof: the ground of ſuch reaſoning, and its extenſive uſe, may be more particularly explained on ſome other occaſion. Suffice it at preſent to add to the note on that article,

That as the given factors (Q and Q^3) in F and x, may be joined with M^n, or with M^{1-n} *indifferently*, the caſe is ſimilar to that of an equal chance at play, for the ſtakes n and $1-n$, where the juſt expectation of each gameſter is $\frac{1}{2}$, whatever be the value of n. The fifth and ſixth Propoſitions of Element I. ſcarce needed any other demonſtration than, that it is manifeſtly impoſſible to aſſign any *reaſon* of inequality, of the angles in one of theſe theorems, and of the ſides in the other. In the following Propoſition, where it is proved, that two lines being extended from the extremities of a right line, A B, to a point C, there is no other point on the ſame ſide of A B, to which lines from A and B equal to the former can be drawn: he who holds the contrary is ſuppoſed to fix upon that other point D; but why D? rather than d, d', d'', &c. he is ſilent: and, therefore, I conclude, there is no ſuch point different from C. And the like may be ſaid of ſome other ſimple theorems, that are commonly demonſtrated by ſhewing the abſurdity of aſſerting their contraries.

 2. The

2. The title of this paper renders it almoſt needleſs to remind the reader, that the moon's parallax is not here propoſed as the propereſt *medium* for determining that of the ſun. Our *data* are ſtill too uncertain for that purpoſe, ſcarce one of them having been determined to an unexceptionable preciſion; and the numbers in the table ſhew how much a ſmall difference in the moon's diſtance muſt affect the ſeveral concluſions. It may be of uſe, however, to know in what manner thoſe concluſions, as well as the quantities from which they derive *, ſtand related to one another. For if, hereafter, the neceſſary *data* ſhould be more exactly known, the calculus may be repeated; and if the tranſit of Venus, which is to happen in 1769, ſhould confirm Mr. Short's calculations from that of 1759, we may thence conclude the true mean diſtance of the moon, better than in any other way.

3. In the mean time, if any perſon ſhould have the curioſity to examine the numbers of the table, he will pleaſe to take notice:

That, as no two meaſurements, nor any two lengths of a ſecond-pendulum hitherto obſerved, make the earth of the ſame ſpheroid figure, I have retained for the ratio of its greateſt and leaſt diameters, that of 231 to 230; anſwering to the hypotheſis of its uniform denſity: and have thence made a degree of the equator equal to 57200 French

* The connexions of F, x, Q, are manifeſt; and the relation of M to Q^2 is eaſily deduced from Prop. 59. Princip. Book I.

† This was computed upon the ſuppoſition, that a degree of the meridian at lat. $49^\circ \frac{1}{3}$ is 57183 toiſes: but if that degree is, by

The

The meaſure of a ſecond-pendulum, at the equator, I had from M. de la Condamine, as was ſaid before: it was corrected for the centrifugal force, and the reſiſtance of the air; and the moon's diſtance, as revolving round the earth at reſt, was corrected for the ſun's diſturbing force: which is done either by diminiſhing that diſtance in the ſubtriplicate ratio of 178725 to 177725; or by diminiſhing the length of the pendulum in the ſimple ratio of theſe numbers. In this laſt correction I was favoured with the advice and aſſiſtance of the Rev. Mr. Price, and of the Aſtronomer Royal, FF. R. S.

a correction of M. Picart's operations, found to be only 57075 toiſes; the diameter of the equator here uſed ought to be diminiſhed by about 18 10000th parts.

 Received

Received December 20, 1767.

V. *Obſervations on the Bones, commonly ſuppoſed to be Elephants Bones, which have been found near the River* Ohio *in* America: *By* William Hunter, *M.D. F.R.S.*

Read February 25, 1768.

NATURALISTS, even thoſe of our own times, have entertained very different opinions concerning foſſil ivory, and the large teeth and bones, which have been dug up in great numbers in various parts of the world.

At firſt, ſome thought them animal ſubſtances, and others mineral. When only a certain number of obſervations had been collected, theſe ſubſtances were determined to be mineral: but, the ſubject having been more carefully examined, they were found certainly to be parts of animals.

After this point was ſettled, a diſpute aroſe, to what animal they belonged. The more general opinion was, that they were bones of the elephant; and the great ſimilitude of the foſſil tuſks to the real elephants teeth gave this opinion conſiderable credit.

It was liable however to great objections: the bones were obſerved to be larger than thoſe of the elephant; and it was thought ſtrange that elephants ſhould have been formerly ſo numerous in weſtern countries, where they are no longer natives, and in cold countries, Siberia particularly, where they cannot now live.

We

We had information from Muſcovy, that the inhabitants of Siberia believed them to be the bones of the mammouth, an animal of which they told and believed ſtrange ſtories. But modern philoſophers have held the mammouth to be as fabulous as the centaur.

Of late years the ſame ſort of tuſks and teeth, with ſome other large bones, have been found, in conſiderable numbers, near the banks of the Ohio, in North America. The French Academicians became poſſeſſed of ſome ſpecimens of them; and having compared them with the bones of real elephants, and with thoſe which had been brought to France from Siberia, and with ſimilar bones found in various other parts, determined, with an appearance of probability on their ſide, that they were elephants bones.

Monſieur Buffon gives us the following account of this deciſion [a]: "All this put together, leaves us
" no longer any room to doubt, that thoſe tuſks
" *(defenſes)*, and thoſe large bones *(oſſemens)*, are
" truly the tuſks and bones of the elephant.
" M. Sloane had ſaid this, but had not proved it.
" M. Gmelin has likewiſe ſaid ſo, and more poſitively;

[a] " Tous cela réuni, fait que nous ne doutons plus que ces
" défenſes & ces oſſemens ne ſoient en effet des défenſes & des
" oſſemens d'éléphant. M. Sloane l'avoit dit, mais ne l'avoit
" pas prouvé. M. Gmelin l'a dit encore plus affirmativement, &
" il nous a donné ſur cela des faits curieux; mais M. Daubenton
" nous paroit être le premier, qui ait mis la choſe hors de doute,
" par des meſures préciſes, des comparaiſons exactes, & des
" raiſons fondées ſur les grandes connoiſſances qu'il s'eſt ac-
" quiſes dans la ſcience de l'anatomie comparée." Hiſt. Naturelle, Tom. XI. p. 87.

"and he has given us ſome curious facts concern-
"ing this queſtion; —— —— but M. Daubenton
"appears to us to be the firſt who has put the mat-
"ter beyond doubt, by accurate meaſures, by exact
"compariſons, and by reaſons founded upon the
"great knowledge which he has acquired in the
"ſcience of comparative anatomy."

From the firſt time that I learned this part of natural knowledge, it appeared to me to be very curious and intereſting; inaſmuch as it ſeemed to concur with many other phænomena, in proving, that in former times ſome aſtoniſhing change muſt have happened to this terraqueous globe; that the higheſt mountains, in moſt countries now known, muſt have lain for many ages in the bottom of the ſea; and that this earth muſt have been ſo changed with reſpect to climates, that countries, which are now intenſely cold, muſt have been formerly inhabited by animals which are now confined to the warm climates.

Some time in the laſt ſpring, having been informed that a conſiderable quantity of elephants teeth had been brought to the Tower, from America; and being deſirous of procuring ſome information concerning them, I waited upon Mr. Bodington, to know the particulars, and to beg leave to examine them. He obligingly gave me a verbal account of their having been brought from the banks of the Ohio; and on the following day ſent me one tuſk, and one grinder, as ſpecimens for my examination. The tuſk, indeed, ſeemed ſo like that of an elephant, that there appeared no room for doubt. I ſhewed it to my brother, and he thought ſo too: but, being particularly converſant with comparative anatomy, at the firſt

first fight he told me that the grinder was certainly not an elephant's. From the form of the knobs on the body of the grinder, and from the difpofition of the enamel, which makes a cruft on the outfide only of the tooth, as in a human grinder, he was convinced that the animal was either carnivorous, or of a mixed kind. This made me think that the tufk itfelf was not a real elephant's tooth: for Mr. Bodington had told me, that there were many grinders, as well as tufks, and that they were all fimilar to thofe fpecimens which he had fent to me. And fome time after, when I went to the Tower, and examined the whole collection which had been fent over from the Ohio, I faw that the grinders were all of the fame kind. I examined two elephants jaws in my brother's collection: I examined the tufks and grinders of the Queen's two elephants: and I examined a great number of African elephants teeth at a warehoufe.

From all thefe obfervations I was convinced that the grinder tooth, brought from the Ohio, was not that of an elephant; but of fome carnivorous animal, larger than an ordinary elephant: and I could not doubt that the tufk belonged to the fame animal. The only difference that I could obferve between it and a real elephant's tufk was, that it was more twifted, or had more of the fpiral curve, than any of the elephants teeth which I had feen.

Some time after this, Dr. Franklin received a large box of the fame fort of bones from the Ohio, by the way of Philadelphia. He informed me of this, and told me likewife that another large box of thofe bones was fent to the Earl of Shelburne, one of his

Majefty's

Majeſty's ſecretaries of ſtate. I waited upon Dr Franklin, with ſome other friends, and found the bones to be exactly ſuch as I had ſeen; and was, therefore, confirmed in my former opinion.

Then I waited upon Lord Shelburne, and was permitted to examine the bones which he had received. Beſides the tuſks and grinders, which were all ſuch as I had ſeen, and ſtill ſerved to confirm me in my opinion, there was the half of the lower jaw of the animal, with one large grinder ſtill fixed in it. This jaw-bone was ſo different from that of an elephant, both in form and in ſize, and correſponded ſo exactly with the other bones, and with my ſuppoſition, that I was now fully convinced, that the ſuppoſed American elephant was an animal of another ſpecies, a *pſeud-elephant*, or *animal incognitum*, which naturaliſts were unacquainted with. I imagined farther, that this *animal incognitum* would prove to be the ſuppoſed elephant of Siberia, and other parts of Europe; and that the real elephant would be found to have been in all ages a native of Aſia and Africa only.

The Earl of Shelburne, from his love of natural knowledge, ſhewed a deſire that the enquiry might be carried on; and did me the honour to offer his aſſiſtance in tranſmitting orders to America, for procuring farther information about this matter. In conſequence of this generous offer, I propoſed that his lordſhip ſhould ſend the following queſtions and orders, to any perſon in America, whom his lordſhip might think the beſt qualified for conducting ſuch buſineſs.

Queries

Queries and orders concerning the bones, called elephants bones, found in the marſh, called the Salt-Lick, near the River Ohio.

I. Do thoſe bones appear to have lain upon the ſurface of the earth from the firſt? Or,

II. Do they ſeem to have been originally at ſome depth in the earth, and to have been afterwards expoſed by the earth's falling away, or by its being waſhed away by floods, &c.?

III. How far is that part of the marſh from the river? How high above the common ſurface of the water of the river? And does it appear probable, from the level and face of that marſh, that in former times the river may have run where the bones are?

IV. How many elephants ſkeletons have been found, as far as may be collected from the number of tuſks, or other marks? and at what diſtance from one another?

V. To ſend over, if poſſible, a whole head, or the moſt entire parts of a head, eſpecially of the upper jaw; and a foot, or the ſmall bones of it, if they can be diſtinguiſhed; and any bones which have thoſe parts pretty entire which once made a joint.

VI. To make correct drawings of any of the bones which are pretty entire, if, on account of their ſize, or tenderneſs, they cannot be ſent over to England.

VII. If the bones do not lie in blended heaps, but thoſe of one ſingle animal all together, and at

at ſome little diſtance from others, it might be of ſervice towards aſcertaining the ſpecies of this animal, to expoſe or uncover one compleat ſett of bones, without moving any one of them from its place; and to make a general drawing of the whole, as they appear in that ſituation; and to ſend as many of them as are tolerably perfect over to England, with that drawing.

Lord Shelburne was pleaſed to take the care of this propoſal upon himſelf; and in proper time will probably receive ſuch information as may be ſatisfactory.

I thought it would be adviſeable, in the mean time, to collect all the information I could upon this ſubject; and to lay the reſult of ſuch enquiries before this Society: that thoſe who may have better opportunities might be invited to the ſubject, and no longer leave ſo capital an article of natural hiſtory uncertain.

I examined all the foſſil teeth, as they are called, in the Muſæum of this Society, and the head and teeth of an hippopotamus. Then, with Dr. Knight firſt, and a ſecond time with Dr. Solander, I examined all the foſſil teeth, and all the jaw-bones, and teeth of elephants, and hippopotami, and other large animals, in the Britiſh Muſæum; and ſome likewiſe in private collections. In making this ſearch, I met with grinders of the *incognitum* that were found in the Brazils and Lima, as well as in different parts of Europe.

At this time Lord Shelburne presented the largest of the American tusks, and the jaw-bone, and some grinders, to the British Musæum; and his Lordship did me the honour to send me the smaller tusk, and two grinders.

I went to four of the principal workers and dealers in ivory, with whom I saw and examined many hundreds of elephants teeth. Though they all assured me, that the real elephants teeth have often a spiral twist, like a cow's horn; they could not shew me one tooth so twisted, in all their collections, at the time when I visited them. Three of them did me the favour to come to my house; and they gave it as their opinion, that my two American tusks were genuine elephants teeth. One of them was even positive that they were African teeth. Another worker in ivory cut through that tusk which Lord Shelburne gave me. It proved to be sound on the inside. He assured me, that it was true elephantine ivory; and that workers in ivory could readily distinguish the genuine, by its grain and texture, from all other bony substances whatever. He polished it: we compared it with other pieces of genuine ivory; and indeed they appeared to be perfectly similar. His opinion was afterwards confirmed by another experienced worker in ivory. Yet their opinion, and what I saw with my own eyes, convinced me of this fact only, viz. that true or genuine ivory is the production of two different animals; and not of the elephant alone.

Having thus collected all the materials to which I could have access, I carefully read what the French Academicians Messrs. Buffon and Daubenton have written on this question, in the *Histoire Naturelle*,

 Tom.

Tom. XI. p. 86. &c. and p. 147. &c. Tom. XII. p. 63.; and *Memoires de l'Acad. Roy. des Sc.* Ann. 1762. p. 206. &c. But, inftead of meeting with facts which could difprove my opinion, I found obfervations and arguments which confirm it. One very material fact, which Mr. Daubenton furnifhes in fupport of my hypothefis, is the comparifon of the American thigh-bone, with that of a real elephant; both of which he has reprefented in figures, which appear to be done with accuracy. To me it feems moft evident, that they are bones of two diftinct fpecies. The vaft difproportional thicknefs of the American bone, compared with that of the elephant, is furely more than we can attribute to the different proportions of bones, in the fame fpecies, which arife from age, fex, or climate. But Mr. Daubenton, to fupport his hypothefis, that the American *femur* is elephantine, is obliged to refer the great difproportion in thicknefs to the caufes above-mentioned; and he affirms that in all other circumftances they are exactly alike. Now, to my eye, there is nothing more evident, than that the two *femora* differ widely in the fhape and proportion of the head; in the length and direction of the neck; and in the figure and direction of the great trochanter: fo that they have many characters, which prove their belonging to animals of different fpecies.

In order to prove to the fatisfaction of the fociety, that the *incognitum* of America is of a very different fpecies from the elephant, I have added three drawings of the jaw-bone of that animal; which the curators of the Britifh Mufæum were pleafed to give me leave to take, and which Mr. Rymfdyk executed with a moft fcrupulous exactnefs: and that the comparifon

II.

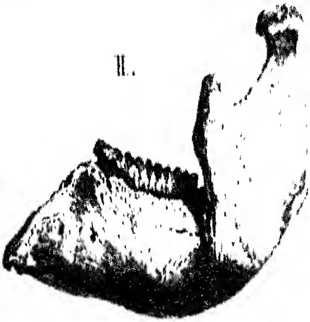
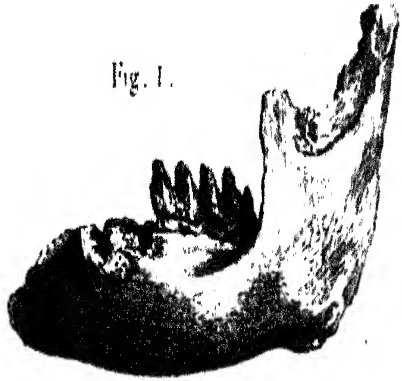
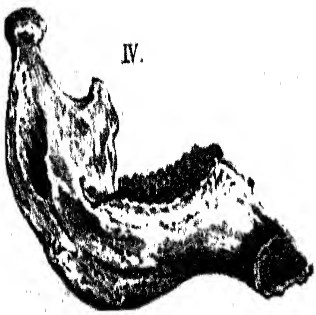


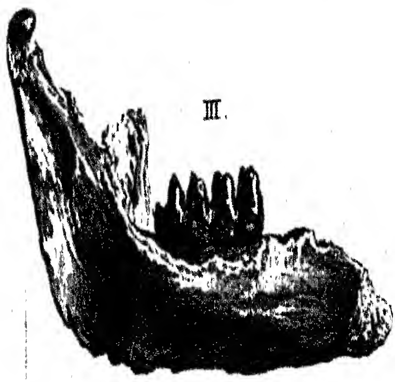
Fig. I.



IV.



III.



VI.

V.

H. Roemsdyk del.

parifon might be made with eafe, I have added three fimilar drawings, taken from the largeft of the two full-grown Elephants jaws which were in my brother's collection; executed with the fame care, by the fame artift; and drawn to the fame fcale, *nine* inches in the real object making *one* in the figure.

TAB. IV. Fig. I. An outfide view of the half of the lower jaw of the American *incognitum*, which the Earl of Shelburne depofited in the Britifh Mufæum. From the top of the condyle to the anterior extremity, the bone meafured, in a ftreight line, thirty five inches: the bafis alone, in a ftreight line, two feet and four inches.

Fig. II. The fame view of the fame bone in a full-grown Elephant, drawn to the fame fcale.

Whoever will take the pains to compare thefe two figures, with a critical eye, will fee that they differ fo very much, not only in fize, but in their general character, and in the particular parts and features, that he cannot entertain a doubt of their being the jaws of two very different animals.

Fig. III. A view of the infide of the fame jaw-bone of the *incognitum*.

Fig. IV. A view of the infide of the fame jaw-bone of the Elephant.

In comparing thefe two views, the difference if poffible is ftill more manifeft.

Fig. V. A view from above of the jaw of the *incognitum*.

Fig. VI. The fame view of the Elephant's jawbone.

It may now be fairly prefumed that the A[illegible] bones are proved to be, certainly, not elephantine:

G 2 and

and whoever is of that opinion, will naturally ſuſpect that the Siberian bones are of the ſame kind. I imagine that it will be found, upon ſtrict enquiry, to be ſo. But, as I have not the neceſſary materials for diſcuſſing this queſtion at preſent, I ſhall only ſtate a few facts, to ſhew that there is ſome ground for the opinion.

1. All accounts, and particularly thoſe of Meſſ. Gmelin, Buffon, and Daubenton, ſay that the bones found in Siberia are larger than the bones of common Elephants. This would make us inclined to ſuſpect that they were not Elephants bones, but that they were of the Incognitum.

2. The Siberian *femur*, as repreſented by Monſieur Daubenton, is *very much* like the American *femur* in ſize, ſhape, and proportions.

This circumſtance appears to be almoſt a demonſtration, as we have before proved, that the American *femur* is not that of an Elephant. And in this argument, we have even the weight of Monſieur Daubenton's opinion in our favour. For he (page 211.) taking it for granted that the Siberian *femur* was undoubtedly elephantine, *reaſons* from the likeneſs in ſize, ſhape and proportions, that the American *femur* is ſo. Now, as we have ſhewn that the American *femur* is not elephantine, his proof taken from the ſize, ſhape, and proportions of the two bones, muſt ſerve to convince us that the Siberian thigh-bone is not of the Elephant, but of the *incognitum*.

3. Monſieur Daubenton found a difference between the temporal bone brought from Siberia, and that of an Elephant. This likewiſe is an argument in favour of our ſuppoſition.

 4. The

4. The ſuppoſed Elephant's tuſk, which was brought from Siberia by Mr. Bell, and preſented to Sir Hans Sloane, and of which we have a deſcription and figure in the Memoirs of the Academy of Sciences at Paris (An. 1727. page 309), is evidently twiſted like the tuſk of the *incognitum*, and not at all like any Elephant's tuſk which I have ever ſeen. This proof will have conſiderable weight with thoſe who will take the trouble to examine that tuſk in the Britiſh Muſæum.

In the laſt place, it may be obſerved, that as the *incognitum* of America has been proved to have been an animal different from the Elephant, and probably the ſame as the Mammouth of Siberia; and as grinder teeth like thoſe of America have been dug up in various other parts of the world; it ſhould ſeem to follow, that the *incognitum* in former times has been a very general inhabitant of the globe. And if this animal was indeed carnivorous, which I believe cannot be doubted, though we may as philoſophers regret it, as men we cannot but thank Heaven that its whole generation is probably extinct.

Received January 11, 1768.

VI. *Obſervations made on the Iſlands of* Saint John *and* Cape Briton, *to aſcertain the Longitude and Latitude of thoſe Places, agreeable to the Orders and Inſtructions of the Right Honourable the Lords Commiſſioners for Trade and Plantations: By Captain* Holland, *Surveyor General in* Canada, *and his Aſſiſtants. Communicated by* R. Brocklesby, *F. R. S.*

Read March 3, 1768.

THE inſtruments made uſe of, in theſe obſervations, were;

I. A monthly aſtronomical clock, or time-piece (with a compounded pendulum, and a ſpring to keep it going when the clock is wound up), made by the late Mr. George Graham.

II. An aſtronomical quadrant, or equal altitude inſtrument of two feet radius, divided by Mr. Siſſon, and improved, with an horizontal circle and ſtand, by Meſſ. Heath and Wing.

III. A two feet Gregorian reflecting teleſcope, made by Mr. Short.

IV. A ten feet refracting teleſcope, reverſing the objects, made by Mr. Dollond.

In

In building my winter habitation on Saint John's Iſland, I conſtructed a ſtrong ſtone chimney; to the back of which I ſecured the clock with the greateſt precaution; and the room was kept temperate by an iron ſtove. In a few days, the clock was regulated to mean or equal time; and always examined and compared by equal altitudes of the ſun and ſtars, at or near the time when any immerſions or emerſions were to be obſerved. As the going of this clock is not inferior to any made by that renowned artiſt Mr. Graham; it will not be neceſſary to inſert here a multitude of equal altitudes of the ſun, and other obſervations, to prove the exactneſs of this clock; but only mention, that I have made uſe of Monſieur De la Lande's Tables * to rectify the equal altitudes of the ſun, for the alteration of the ſun's declination during the time of obſervations.

1765.

1 Obſer. January 20. Obſerved an emerſion of the firſt ſatellite of Jupiter, at 7 hours, 42 minutes, 3 ſeconds, equal or mean time. Mr. Haldimand, who obſerved with Mr. Dollond's teleſcope, perceived the ſatellite two ſeconds later than I did with Mr. Short's; the latter having the ſecond power magnifying 150 times. As this was the firſt obſervation, we began too ſoon to obſerve, and fatigued our eyes ſo much, that we were not ſure to a few ſeconds.

* See Monſieur De la Lande's *Aſtronomie*, Article 637, page 279.

2 Obſer.

2 Obſer. January 27. The firſt ſatellite of Jupiter emerged at 9 hours, 36 minutes, 31 ſeconds, mean or equal time. By bringing Mr. Short's teleſcope out of a warm room, into the exceſſive cold air, the mirror became very dull, by which I loſt my obſervation; but Mr. Haldimand was ſure of his being good.

3 and 4 Obſer. March 23. The third ſatellite of Jupiter in immerging began to appear faint at 9 hours, 4 minutes, 48 ſeconds, and intirely immerged at 9 hours, 5 minutes, 47 ſeconds; and in emerging began to appear faintly at 12 hours, 27 minutes, 52 ſeconds, and fully recovered its luſtre at 12 hours, 27 minutes, 58 ſeconds, equal or mean time. Obſerved by us both.

5 Obſer. April 6. The firſt ſatellite of Jupiter in immerging appeared faintly at 10 hours, 15 minutes, 54 ſeconds, equal time, and recovered its full luſtre in 4 ſeconds afterwards. Obſerved by us both.

6 Obſer. April 21. The firſt ſatellite of Jupiter emerged at 8 hours, 34 minutes, 51 ſeconds, and recovered its full luſtre in 3 ſeconds afterwards. Obſerved by us both.

7 Obſer. April 29. The firſt ſatellite of Jupiter emerged at 10 hours, 29 minutes, 30 ſeconds, equal time. Obſerved with Dollond's teleſcope only.

By the mean of all the meridian altitudes of the ſun and ſtars; the latitude of this place is 46° 2′ 30″ north.

In the beginning of May, I went on the ſurvey of Saint John's Iſland; and lieutenant Haldimand to

to furvey and fettle the latitude and longitude of the Magdalen Iflands.

The longitude, lieutenant Haldimand determined by the mean refult of feveral obfervations of the diftance of the Moon, from the Sun and fixed ftars.

This gentleman would have given a full account of his obfervations, with fome natural remarks on the Sea Cows, which in prodigious numbers refort to that Ifland, had he not unfortunately loft his life, being drowned prefently after his return to Louifburg.

The refult of his obfervations is;

The Magdalen Iflands are fituated in the Gulph of St. Lawrence, in the latitude of 47° 41′ North, and between 61° and 61° 38′ Weft longitude from London. The variation of the Compafs is 17° 30′ Weft.

The Ifland of Entry, lies in 47° 17′ North latitude, and 61° 20′ Weft longitude from London.

The Bird Iflands are in 47° 55′ North latitude, and bear from the Eaft point of Magdalen North 35° Eaft, diftant 18 Miles.

Bryon Ifland is in 47° 52′ North latitude, and the Eaft point bears North 13° Weft of the Eaft point of Magdalen; diftant 12 miles.

At my arrival at Louifburg, the 26th October 1765, I brought the inftruments in order; fixed the clock to a brick wall, in a room kept warm by a ftove, and regulated it to equal or mean time.

I alfo put up in a different room, near the fire place, another monthly clock, fent me from London, made by Meff. Mudge and Dutton, with a fimple or common pendulum; this clock, with little trouble, was foon brought to keep time with the other clock, though, when

when the fire was neglected, it was soon perceived in its motion.

The weather here proved very disadvantageous for astronomical observations, as well by the continual fogs, as severe frost.

1766.

1 Obser. March 10. Observed by Mr. Thomas Wright and me; the first satellite of Jupiter emerged at 11 hours, 21 minutes, 27 seconds, equal or mean time. This observation was made with the aforementioned instruments, and in the same manner. This day I had taken several equal altitudes to examine the clock, and found it 4 seconds too slow of equal time.

April 7. We discovered a Comet at 8 hours, 52 minutes; the tail of which was perpendicular to the horizon, with its head towards the Sun: the light of it was very pale, and it set behind the hills at 9 hours, 30 minutes. As near as I could remember, its position, when I came to look on the globe, was between the tail of Aries and Musca; we made preparations to make proper observations on the night ensuing; but a fog, which continued for several days, prevented us; and when it cleared up, the comet had so much approached the Sun, that we never after could see it.

2 Obser. April 14. The third satellite of Jupiter emerged at 11 hours, 51 minutes, 47 seconds; but some flying clouds made this observation a little dubious, which also occasioned losing the immersion of the same satellite. Observed by us both.

3 Obſer. April 25. The firſt ſatellite of Jupiter emerged at 11 hours, 46 minutes, 50 ſeconds. Obſerved by us both.

May 1. The ſeveral ſurveying parties diſperſed round this Iſland, to finiſh the ſurvey, and take the latitudes of all the moſt remarkable places; of which the reſult is;

At Louiſburg and Iſland Battery, the latitude is 45° 54′ North.

Cape North, latitude 47° 2′ North.

St. Paul's Iſland, North cove, latitude 47° 11′ North.

The entrance of Dartmouth Harbor or *Baye des Eſpagnols*, latitude 46° 13′ North.

——Conway or St. Anne's Harbor, latitude 46° 20′ North.

The North head of Colviles Bay, or Niganiche, latitude 46° 44′ North.

Auguſt 4. I returned to Louiſburg, to obſerve the eclipſe of the Sun on the day following. This eclipſe appeared here perfectly annular; but I could only obſerve the undermentioned circumſtances, as my large teleſcope and micrometer were out of order.

I obſerved with the teleſcope fixed to my quadrant or equal altitude inſtrument of two feet long reverſing the objects; and Mr. Thomas Watts obſerved with one of Dollond's, four feet long.

During the time of obſervation, the ſky was very clear, and I took ſix equal altitudes, to examine the clock.

	h	′	″	
The beginning of the eclipſe	12	38	40	Equal, or Mean Time.
The ring formed	2	8	48	
The ring opened	2	12	6	
End of the eclipſe	3	33	50	

 From

1767.

From this time I was prevented, either by the business of the survey, or by the inconveniency of the weather, to make any further obſervations, until February 18.

1 Obſer. The firſt ſatellite of Jupiter immerged at 11 hours, 47 minutes, 10 ſeconds, mean time.

2 Obſer. February 25. The ſame ſatellite of Jupiter immerged at 13 hours, 41 minutes, 14 ſeconds, mean time.

3 Obſer. February 27. The ſame ſatellite of Jupiter immerged at 8 hours, 10 minutes, 17 ſeconds, mean time.

4 Obſer. March 6. The ſame ſatellite of Jupiter immerged at 10 hours, 4 minutes, 29 ſeconds, mean time.

5 Obſer. March 15. The ſame ſatellite of Jupiter immerged at 8 hours, 41 minutes, 19 ſeconds, mean time.

6 Obſer. April 7. The ſame ſatellite of Jupiter emerged at 8 hours, 53 minutes, 11 ſeconds, mean time.

7 Obſer. April 14. The ſame ſatellite of Jupiter emerged at 10 hours, 47 minutes 44 ſeconds, mean time.

The obſervations of the immerſions and emerſions were this year made with Dollond's refracting teleſcope; the reflecting teleſcope of Mr. Short being ſent with Mr. Wright (one of my deputy ſurveyors), who went laſt Auguſt to the Iſland of Anticoſti, to ſurvey, make obſervations for the aſcertaining the longitude and latitude, and keep a meteorological journal.

I hope

I hope ſome of Mr. Wright's obſervations will be made on the ſame time with mine, that I may be able to ſettle immediately the longitude of the Iſland of Anticoſti, and the entrance of the river of St. Lawrence. But to determine with exactneſs the longitudes hitherto taken, it will be neceſſary to deliver a copy of this paper to ſome members of the Royal Society, that they may be compared with correſponding obſervations, made in England.

By order of the Surveyor General.

George Derbage, Sec.

Received January 14, 1768.

VII. *A Note concerning the Cold of* 1740, *and of this Year.* *By* J. Bevis, *M. D. F. R. S.*

Read March 3, 1768.

I Find in my journal of aſtronomical obſervations made at Stoke Newington, in a detached or inſulated obſervatory, whoſe walls were of brick and two feet thick, that during moſt part of the night of the 5th day of January $17\frac{39}{40}$, the ink in my ſtand-diſh would freeze in a few minutes, if brought within a foot of the wall; and that the ſurface of the water wherein the ball of the plumb-line hung, for rectifying the poſition of my mural quadrant, was continually freezing, ſo that I was obliged to thaw it frequently, by pouring in hot water; yet was there a good fire in the room all the night. At 5 in the morning, of the 6th, a Fahrenheit's thermometer, made by himſelf, expoſed to the North, ſtood ſomewhat lower than 10, that is, more than 22 diviſions below freezing. This was the coldeſt night of that year there.

This preſent year, in Brick-court, N° 1, Middle Temple, the ſame thermometer, expoſed out o'doors to the North, ſtood loweſt on New year's day in the morning, to wit at 17, and once again at the ſame place; but then I am to obſerve that I am on that ſide my chambers environed with buildings almoſt contiguous to the wall my inſtrument is hung againſt, wherein very probably fires were kept up, which hindered the quickſilver from ſinking conſiderably lower.

J. Bevis.

Received February 4, 1768.

VIII. *Obſervations on the ſame Subject, by* J. Short, *F. R. S.*

Read March 3, 1768.

IN the year 1738, I bought of Mr. Scarlet a diagonal barometer. On the upright part of this barometer is affixed a ſpirit of wine thermometer, ſaid to have been made according to the ſcale of the Royal Society's thermometer. On Tueſday, the 25th of December 1739, the great froſt of that and the ſucceeding year began. On Saturday the 19th of December, at one o'clock in the afternoon, I obſerved that the ſpirits of wine were deſcended ſo low as not to be ſeen, that is to ſay, were below the top of the wood which covers the ball of the thermometer. I had another thermometer of quickſilver, made alſo after the ſcale of the Royal Society's thermometer: I found that the quickſilver of it was ſunk within the ball. This laſt thermometer has been broke many years ago. The ſpirit of wine thermometer I have ſtill in my poſſeſſion, in the ſame condition it was then. This thermometer ſtood then, as it does now, within a room next to the river, at the greateſt diſtance from two windows, one of which looks South, the other Weſt. The wind blew hard from the Eaſt on the 29th of

of December. There was then no ice on the river, which I could ſee.

I have ſince that time ſettled the freezing point of this thermometer. I find that the ſpace, on this thermometer, contained between the freezing point and the top of the wood which covers the ball, is equal to 21 diviſions of Fahrenheit's ſcale. It, therefore, follows that the cold, at one of the clock in the afternoon on the 29th of December 1739, was ſo great as to ſink the mercury 21 diviſions below the freezing point of Fahrenheit's ſcale, within this room, the windows being ſhut.

On the 31ſt of December 1767, at 8 o'clock in the morning, a Fahrenheit's thermometer without the window of the ſame room, where it had remained all night, ſtood at 19¾ diviſions below the freezing point; but a ſimilar Fahrenheit's thermometer within the room ſtood at 13¾ diviſions below the freezing point; therefore the cold, that morning, was greater without the room than within it by 6 diviſions of Fahrenheit's ſcale.

On the 7th of January 1768, at 8 o'clock in the morning, a Fahrenheit's thermometer without the window of the ſame room, where it remained all night, ſtood at 19¾ diviſions below the freezing point; but a ſimilar Fahrenheit's thermometer within the room ſtood at 12 diviſions below the freezing point; therefore the cold, that morning, was greater without the room than within it by 8 diviſions of Fahrenheit's ſcale.

From what has been ſaid, I think, we may ſafely conclude, that the cold, on the 28th of December 1739 at one o'clock in the afternoon, was ſo great, without the

the window of the ſaid room as to ſink the mercury 27 diviſions, at leaſt, below the freezing point of Fahrenheit's thermometer.

N. B. No fires were made in the ſaid room, or in the two contiguous rooms, in the year 1739, or in the years 1767 and 1768.

Surry Street, 3d February, 1768.

J. Short.

Received January 18, 1768.

IX. *An Investigation of the Difference between the present Temperature of the Air in* Italy *and some other Countries, and what it was Seventeen Centuries ago: In a Letter to* William Watson *M. D. F. R. S. by the Honourable* Daines Barrington *F. R. S.*

Dear Sir,

Read March 3, 1768.

I Troubled you lately to procure from your Son Mr. Watson, who is now in Italy, some answers to certain queries with regard to the temperature of the seasons which commonly prevails there at present, and particularly whether the rivers of that country have been remembered to have had their surface frozen over.

I have often put the same question to many of those who have made the tour of Europe, and have always been answered in the negative: as most Englishmen however travel before they pay attention to facts of this sort, I was desirous of procuring information from those whose observations might be more depended upon.

I have long entertained a notion that the seasons are become infinitely more mild in the Northern latitudes than they were 16 or 17 centuries ago; and from this it hath happened that many passages in the classical writers descriptive of the Severity of the climates, have struck me more than they would perhaps a common reader.

 It

It will be immediately ſeen that the proofs of this aſſertion muſt depend upon accounts of the weather, and it's effects in places, the ſituation of which we know with ſome preciſion, and which may be compared with the common meteorological obſervations in the ſame latitudes and ſpots at preſent.

If this ſame queſtion ſhould be agitated two thouſand years hence, it might receive an abſolute demonſtration; as a journal of the changes in a well-conſtructed thermometer would ſhew the temperature which prevailed in any particular place, during the preſent century.

No ſuch accuracy can be expected from any paſſages in the claſſical writers; but in order to ſtate the alteration which may have happened in ſo long a courſe of years, the moſt proper method ſeems to be to compare their accounts with thoſe of more modern travellers, who have equally wanted the aſſiſtance of a thermometer for their obſervations.

I ſhall for ſeveral reaſons chiefly rely upon many of Ovid's letters from Pontus (though he was not only a poet, but a writer of moſt glowing fancy, and imagination), in which he deſcribes the effects of cold at Tomos * during his ſeven years reſidence there, and afterwards contraſt this deſcription with that of later travellers.

* It is ſo called by Ovid, who reſided there ſo long and underſtood the language of that country. It was however likewiſe ſtyled Tomis and Tomi, the latter of which ſeems to have been the more general appellation, as the adjective formed from it is Tomitanus. Beſides this, Ferrarius ſuppoſes it to be the ſame with the modern Temiſware, moſt evidently taken from the ancient name in the time of Ovid.

Ovid was born at Sulmo in Italy, about ninety Roman miles S. W. from the capital:

"Millia qui decies diſtat ab urbe novem."

He afterwards reſided chiefly at Rome, and was there at the time he received the Emperor's orders for his immediate baniſhment: I ſhall therefore conſider him as then leaving the 42d degree of Northern latitude, the climate in which he was born, and continued to live.

He was thence removed to Tomos, which Dr. Wells, in his maps of ancient geography, places only in the 44th degree of Northern latitude: the change was therefore only of two degrees, and yet Ovid immediately deſcribes the winter of Hudſon's bay.

But, before I particulariſe any of the paſſages which prove the intenſeneſs of the cold, which he there experienced, it may be objected that no credit is to be given to a melancholy poet, of a warm imagination and too exquiſite feelings.

This argument, I admit, would have great weight, if he only complained of the exceſſive and intolerable cold which prevailed. The maxim of law, however, holds equally in natural philoſophy* "*that he who "means to impoſe or miſrepreſent never deals in particu-"lar facts,*" eſpecially ſuch as admit of an immediate contradiction, and in which he could not himſelf be deceived.

He ſaw with his own eyes the Euxine ſea covered with ice:

"Vix equidem credar, ſed cum ſint præmia falſi
"Nulla, ratam teſtis debet habere fidem:
"Vidimus ingentem glacie conſiſtere Pontum."

Lib. III. El. 10.

* "Fraus verſatur in generalibus."

But if you will not credit what he ſaw, he afterwards mentions walking upon this ice:

"Nec vidiſſe ſat eſt, udum calcavimus æquor;
"Undaque, non *udo ſub pede*, ſumma fuit."

The ſea thus frozen, not only bore Ovid who hath deſcribed himſelf to be very light and agile, but oxen and carriages paſſed over it:

"Perque novos pontes, ſubter labentibus undis,
"Ducunt Sarmatici barbara plauſtra boves."

Lib. III. El. 10.

When the poor baniſhed poet, during this rigorous weather, wanted ſome generous wine to warm himſelf, it was preſented to him in a ſtate of congelation:

"Udaque conſiſtunt formam ſervantia teſtæ
"Vina, nec hauſta meri, ſed data fruſta, bibunt."

This effect of cold was not experienced in London, ſituated in the 52d degree of Northern latitude, during the great froſt in 1740.

Add to theſe proofs, that what he here mentions is not the effect of one particular hard and ſevere winter; he complains from year to year of nearly the ſame circumſtances:

"Ut ſumus in Ponto *ter* frigore conſtitit Iſter,
"Facta eſt Euxini *dura ter* unda maris."

Lib. I. El. 10.

The ſnow likewiſe in many places never diſſolved during the ſummer:

"Quæque nec hoſte fero, nec nive terra cares."

"Frigore perpetuo Sarmatis ora riget.

"Et ſolet in multis *bima* manere locis."

I think therefore that what I have preſumed to conjecture, may be reſted upon the ſingle teſtimony of

of Ovid; but, as there is with ſome ſo ſtrong prejudice againſt facts from ſuch a quarter, I ſhall endeavour to corroborate this authority, by deſcriptions of the ſame country, which we find in other writers of thoſe centuries.

His contemporary Virgil ſpeaks thus of the effects of cold in the ſame latitude:

"Cæduntque ſecuribus humida vina,
"Stiriaque impexis induruit horrida barbis."

Georg. Lib. III. l. 349. & ſeq.

He likewiſe aſſerts that the ſnow.

"——— Septem aſſurgit in ulnas."

Virgil indeed is alſo unfortunately a poet; but his Georgics are perpetually relied upon as authority, not only by Pliny, but the later writers on huſbandry.

Such credit is given to our own great deſcriptive poet Thomſon, that the compilers of the Encyclopedie have almoſt entirely tranſlated his *Seaſons*, under the article *Zone:* nor is there perhaps a circumſtance mentioned throughout thoſe poems, which the moſt ſcrupulous, and minute naturaliſt may not rely upon.

I ſhall juſt mention the authority of one more poet, as he is ſcarcely more than a metrical geographer: Dionyſius, in deſcribing the ſame country, ſpeaks thus of the ſnow's never melting:

Σχετλιοι οι περι κεινον ενοικια Φωτες εχουσι.
Αιει σφιν ψυχρη τε χιων, δρυμος τε δυσαης.

Περιηγησις, l. 668.

I ſhall cloſe theſe corroborating proofs of the cold which was experienced at Tomos, by a paſſage from another geographer, who is ſufficiently proſaic not to admit of any objection, to his teſtimony, on account of a too lively imagination:

Απασα

Απασα δη χωρα δυσχειμερος εστι, των δε παγων η σφοδροτης μαλιστα εκ των συμβαινοντων περι το στομα της Μαιωτιδος δηλος εστι, αμαξευεται γαρ ὁ διαπλυς, ὡστε και πηλον ειναι, και οδον. Strabo, L. 7.

Thus is Ovid ſupported in that very material and ſtriking fact of the ice being commonly ſtrong enough to bear carriages.

It now remains to compare this account of the ſeverity of the cold at Tomos, with that of more modern travellers, who have either been at the ſame place during the winter, or paſſed not very far diſtant.

Rubruquis, Marco Polo, Jean du Plan, Carpin, and Mandeville, were all of them on the borders of the Euxine Sea, and proceeded many degrees Northward; and yet we do not hear of any complaint with regard to the cold.

Buſbequius travelled from Buda to Conſtantinople, in the midſt of winter; nor does he mention any inconvenience, or interruption, from froſt or ſnow. If it be ſaid that his way did not lye through Tomos (or Temeſware), to this it may be anſwered, that he croſſed the ſame latitudes; to which it may be added, that there was no Euxine Sea to mitigate the ſeverity of the cold.

I muſt likewiſe here make another obſervation, that it does not appear either from Ovid, who is ſo very minute in every particular relative to this country, or from any other traveller, that there are high mountains in the neighbourhood of Tomos.

Tournefort was on the Black Sea, in the beginning of April, and dwells much upon the very fine weather during the time he continued upon it. He obſerves, however, that, in the time of Conſtantine, the ſtreight oppoſite to Byzantium was frozen over; and that

that in the year 401 the Euxine Sea was covered with ice, for twenty days together. Theſe facts, therefore, ſtruck him, as extraordinary.

Mottraye was in Crim Tartary, in the year 1711, during the months of November and December; who is alſo entirely ſilent, with regard to any uncommon effects of cold.

Theſe are all the travellers, whoſe works I have looked into, or could procure on this occaſion. I do not take upon myſelf to ſay, that there may not be others, which have eſcaped me; but I ſhould not ſuppoſe the number to be great, as the Euxine Sea, and its neighbourhood, neither anſwers to the European traveller, in point of curioſity or commerce.

I have ſaid in the outſet, that I have ſome particular reaſons, for fixing chiefly on Tomos, to make this compariſon; which ariſes from the country being preciſely in the ſame ſtate that it was in the time of Ovid; this entirely excludes the common obſervation, *that the cultivation of a country will render the climate more temperate.*

We will now leave Tomos, and compare the accounts of the weather in Italy, with thoſe of the preſent times: it being firſt premiſed, that the country was better cultivated, in the Auguſtan age, than it is now, which ſhould conſequently have made the temperature of the air more warm than it is now experienced to be.

The queries propoſed to your ſon Mr. Watſon relate to this compariſon, and have occaſioned my troubling you with this length of letter, ſince I have within theſe few days been fortunate enough to procure, through other hands, the information I could have wiſhed on this head.

I ſhall

I ſhall begin with ſome paſſages from Virgil's Georgics, having already attempted to ſhew that no authority can be more relied upon.

This moſt excellent huſbandman is conſtantly adviſing precautions againſt ſnow and ice in the management of cattle; and he may be generally ſuppoſed to give theſe directions for the neighbourhood of Naples *, or Mantua his native country, where he does not evidently from the context mean ſome other parts of Italy:

" Et multâ duram ſtipûlâ, filicumque maniplis
" Sternere ſubter humum, glacies ne frigida lædat
" Molle pecus." Lib. III. l. 297.

This relates to ſheep; but that hardy animal the goat wanted the ſame attention during the winter:

" Ergo omni ſtudio, glaciem, ventoſque nivales
" Avertes." Lib. III. l. 317.

Speaking afterwards of Calabria, the moſt Southern part of Italy, he expreſſes himſelf, with regard to the rivers being frozen, as what was commonly to be expected:

" Et cum triſtis hyems etiamnum frigore ſaxa
" Solveret, & glacie curſus frænaret aquarum †."

* " Illo Virgilium me tempore dulcis alebat
" Parthenope, ſtudiis florentem ignobilis otî."

† It appears alſo by the ſixth Satire of Juvenal, that the Tiber's being commonly frozen in winter ſupplied the ladies of Rome with a very extraordinary inſtance of implicit deference to the commands of the Egyptian prieſts:

" Hybernum fracta glacie deſcendet in amnem,
" Ter matutino Tiberi mergetur ———"

Pliny's favourite villa of Laurentinum was ſituated near the mouth of the ſame river; and, in the very minute deſcription of its beauties and conveniences, he dwells much more upon the expoſition of different parts of it to the warmth of the ſun,

I am aſhamed to be obliged to ſtate ſo many authorities; but, as the proof entirely ariſes from many ſuch concurrent paſſages, I ſhall now ſupport the teſtimony of Virgil by that of two naturaliſts, who were either Italians, or reſided in Italy.

Pliny, in a chapter, *De natura cœli ad arbores*, and ſpeaking of Italian trees, ſays, "Alioqui arborum "frugumque communia ſunt, *nives diutinas ſedere.*" Lib. XVII. cap. 2.

But perhaps the ſtrongeſt proof of that very remarkable fact, the Italian rivers being conſtantly frozen over, is to be collected from a chapter in Ælian, which conſiſts entirely of inſtructions how to catch eels, whilſt the water is covered with ice: to this, without troubling you with a long citation, I ſhall barely refer. (See Lib. XIV. de Animal. cap. 29.)

Now, if we may believe the concurrent accounts of modern travellers, it would be almoſt as ridiculous to adviſe a method of catching fiſh in the rivers of Italy, which depended entirely upon their commonly being frozen over, as it would be to give ſuch directions to an inhabitant of Jamaica.

I likewiſe cannot find that the precautions, which Virgil gives in his Georgics, againſt the damage which ſheep and goats might receive from the ſnow and froſt, are now neceſſary; and both theſe animals are known to ſtand the ſevereſt winters of the High-

than its coolneſs, which is the circumſtance moſt attended to, even in our northern climate.

He alſo mentions, that the ſituation was not warm enough either for olives or mirtle; and that the *laurus* (which, whether it be the *bay* or *laurel*, bears our climate, except in ſeaſons of extraordinary ſeverity) would not then frequently ſtand the whole winter, neither at Laurentinum, nor near the town of Rome.

lands

lands of Scotland, conceived to be in Virgil's time almoſt the *ultima Thule*.

I do not pretend however to aſſert from this, that ſnow does not lye upon the Alps and Apennines, which ariſes from their very conſiderable height, nor that ſome waters on the tops of ſuch mountains, or perhaps nearly under them, may not be frozen, eſpecially when they are at a great diſtance from the ſea.

We ſhall find however that the climate hath likewiſe, upon theſe very ſummits, become proportionably mild with that of the more level countries.

This receives a very clear and ſatisfactory proof, from the difficulties which thoſe formerly encountered who paſſed the Alps.

Every line, almoſt, of both Livy's and Polybius's deſcription of Annibal's paſſage, makes mention of froſt and ice; we know that theſe mountains have been eaſily croſſed ſince by armies, from the time of Francis the firſt, to the war of 1743. They are likewiſe paſſed ſometimes even by conſumptive travellers during the winter.

It is now time, perhaps, to releaſe you from this very long diſſertation; on a point, however, which ſeems to be of ſome curioſity and importance. I am,

Dear Sir,

Your moſt faithful,

humble ſervant,

Daines Barrington.

X. *An Account of Rings consisting of all the Prismatic Colours, made by Electrical Explosions on the Surface of Pieces of Metal, by* Joseph Priestley, *LL. D. F. R. S.*

Read March 10, 1768.

IT was a discovery of Sir Isaac Newton, that the colours of bodies depend upon the thickness of the fine plates which compose their surfaces. He has shown that a change of the thickness occasions a change in the colour; differently coloured rays being thereby disposed to be transmitted through the plate, and consequently rays of different colours being disposed to be reflected at the same place, so as to present the appearance of different colours to the eye.

A variation in the density of the plate, he shows, will occasion a variation in the colour; but still a medium of any density would exhibit all the colours according to the thickness of the different parts of it. These observations he confirmed by experiments on plates of air, water, and glass. He also mentions the colours which arise on polished steel, by heating it; as likewise on bell-metal, and some other metalline substances, when melted and poured on the ground, where they may cool in the open air: and he ascribes these colours to the *scoriæ*, or vitrified parts of the metal, which, he says, most metals, when heated, or melted, do continually protrude, and send out to their surface, covering them in the form of a thin glassy skin. *Optics*, pag. 194.

This capital discovery, concerning the colours of bodies depending upon the thickness of the fine plates

plates which compose their surfaces, of whatever density those plates be (and which may be of such admirable use to explain the colours, and perhaps, in due time, the constituent parts and internal structure of natural bodies) I have been so happy as to hit upon a method of illustrating and confirming, by means of electrical explosions. These, being received upon the surfaces of all the metals, change the colour of them, to a considerable distance round the spot on which they are discharged, so that the whole space is divided into a number of concentric circular spaces, each exhibiting all the prismatic colours; and perhaps as vivid as they can be made in any method whatever.

It was not by any reasoning *a priori*, but by a mere accident, that I first discovered these colours. Having occasion to take a great number of explosions, in order to ascertain the lateral force of them; I observed that a plate of brass, on which they were received, was not only melted, and marked with a circle, by a fusion round the central spot, but likewise tinged, beyond this circular spot, with a green colour, which I could not easily wipe out with my finger. Struck with this new appearance, I replaced the apparatus, and continued the explosions; till, by degrees, I perceived a circle of red beyond the fainter colours; and, examining the whole with a microscope, I plainly distinguished all the prismatic colours, in the order of the rainbow. The diameter of the red, in this instance, happened to be one third of an inch, and the diameter of the purple about one fourth.

Pleased with this experiment, I afterwards pursued and diversified it in a great variety of ways, the result of which I shall comprise in the following observations.

1. When

1. When a pointed piece of metal is fixed opposite to a plain surface, the nearer it is placed to the surface, the sooner do the colours appear, the closer do the rings succeed one another, and the less space they occupy; as, on the other hand, the farther it is placed from the surface, the later do the colours appear; but the rings then occupy a proportionably greater space, and have more room to expand themselves. N° 1. on the steel *, was made by the explosions passing from the point of a needle, fixed at the distance of $\frac{2}{25}$ of an inch from the steel; and N° 2. was made at the same time, when the needle was placed at the distance of $\frac{1}{25}$ of an inch. It seems, however, that when the point is placed at such a distance, as that the electric matter has room to dilate, and form as large a circular spot as the battery will admit, the rings are as large as they are capable of being made; but that still the colours appear later, in proportion to the distance beyond that. When the point is fixed exceeding near, or is made to touch the surface, the colours appear at the very first explosion, but they spread irregularly, and make not distinct rings, as N° 1. upon the tin.

2. The more acutely pointed is the wire, or needle, from which the electric matter issues, or at which it enters, the greater number of rings appear. A blunt point makes the rings larger, but fewer; and in that circumstance it is likewise much later before the colours make their appearance at a given distance. N° 3.

* All the coloured rings mentioned in this paper were shewn to the Royal Society, but could not be well represented by a print.

upon

upon the ſteel, was made by a blunt wire, and N° 2. upon the tin by a braſs knob fixed oppoſite to it.

3. In making theſe rings, the firſt appearance is a duſky red, about the edges of the circular ſpot; preſently after which (generally after four or five ſtrokes) there appears a *circular ſpace*, viſible only in a poſition oblique to the light, and looking like a ſhade on the metal. This ſpace expands very little during the whole courſe of the exploſions, and it ſeems to be, as it were, an attempt at the firſt and fainteſt red; for by degrees, as the other colours fill the bulk of that ſpace, the edges of this ſhade deepen into a kind of brown; as may be ſeen particularly in N° 4. upon the ſteel, where it is ſomething more than half an inch in diameter, and in N° 1. where it is near $\frac{3}{4}$ of an inch.

4. After a few more exploſions, a ſecond circular ſpace is marked out by another ſhade, beyond the firſt, generally about $\frac{1}{8}$ or $\frac{1}{10}$ of an inch in diameter, which I have never obſerved to change its appearance, after ever ſo many exploſions. This ſecond ſhade, by ſucceeding the firſt; which as I obſerved, becomes gradually of a brown, or a light red, ſeems to be an attempt at the fainter colours, which intervene between the reds.

5. All the ſtronger colours make their firſt appearance at the edges of the circular ſpot; and more exploſions make them continually expand towards the extremity of the ſpace firſt marked out, while others ſucceed in their place; till, after about thirty or forty exploſions, three diſtinct rings generally appear, as in N° 4. upon the ſteel. If the exploſions be continued farther, the circle becomes leſs beautiful, and leſs diſtinct;

tinct; the red commonly prevailing, and suffusing all the other colours, as in N° 1. upon the steel; though I attribute the confusion of the colours in that circle, in part, to the needle having been several times accidentally broken from the cement which supported it, and to its not having been replaced exactly as before.

6. The last formed colours are always the most vivid, as appears very distinctly in the reds of N° 1 upon the steel. Also the last formed rings lie closer to one another than the first.

7. These rings may be brushed with a feather, and even wetted, or a finger may be drawn over them, without their receiving any injury; but they easily peel off, when scratched with one's nail, or any thing that is sharp, the innermost rings being the most difficult to erase.

8. The first circles are sometimes covered with a quantity of black dust; part of which however may be wiped off with a feather, so as to show the colours under it. An attempt to wipe off more, on the rough side of the steel, took off the colours along with it; but more than half yet remains, with the dust upon it, as it was first formed.

9. It makes no difference whether the electric matter issue from the pointed body upon the plate, or from the plate upon the pointed body; the plate opposed to the point being marked exactly alike in both cases. Also the points themselves, from which the fire issues, or at which it enters, are coloured to a considerable distance, often about half an inch, but not very distinctly. The colours likewise return here, in concentric rings, as upon the plate.

10. I think

10. I think that the more circles are made at the ſame time, the more delicate will the colours be; whereas the ſurface is, as it were, torn, or corroded by more violent exploſions; which makes the colours appear rough and courſe. N° 4. is I think on this account, as well as ſome others, marked in a more delicate and beautiful manner than N° 1. or N° 5. But this roughneſs is only perceived on the ſteel. On ſilver, tin, and poliſhed braſs, the colours were always free from that roughneſs.

11. A poliſhed ſurface is not neceſſary, the colours being very manifeſt on the rough ſide of the ſteel, where it is not covered with the black duſt mentioned above.

12. Theſe coloured rings appear almoſt equally well on all the metals on which I have made them; namely, gold, ſilver, copper, braſs, iron, lead, and tin.

I have not tried any of the ſemi-metals; but I have no doubt of their anſwering as well as the proper metals.

13. When the pointed wire was made to incline to the plane on which the colours were exhibited, the circular ſpot was quite round, the center of it being in the perpendicular let fall from the point; but the colours were projected oppoſite to the point, in an oblong figure.

Upon ſhewing theſe coloured rings to Mr. Canton, I was agreeably ſurpriſed to find, that he had, likewiſe, produced all the priſmatic colours from all the metals, but by a different electrical proceſs. His method had been to extend fine wires over the ſurface of pieces of glaſs; and when the wire was exploded, he obſerved that the glaſs remained tinged with

with all the colours from all the metals. They are not indeed difpofed in fo regular and beautiful a manner as in the rings I produced; but they equally demonftrate, that none of the metals difcovers the leaft preference to any one colour more than another. A variety of other very extraordinary appearances occurred in the courfe of Mr. Canton's experiments in melting wires.

In what manner thefe colours are formed, it may not be eafy to conjecture. In Mr. Canton's method of producing them, the metal feems to be difperfed in all directions from the place of explofion, in the form of fpheres, of a very great variety of fizes, tinged with all the variety of colours, fome of them too fmall to be diftinctly vifible by any magnifier. In my method, it fhould rather feem that they are produced in a manner fimilar to the production of colours on fteel &c. by heat *i. e.* the furface is affected, without the parts of it being removed from their places, certain plates only, or *laminæ*, being formed, of a thicknefs proper to exhibit the refpective colours at certain diftances; and that the thicknefs of thefe plates is continually changing by the repetition of the explofions.

N. B. The *battery* made ufe of in the abovementioned experiments was of *twenty one fquare feet* of coated glafs.

Received

Received March 9, 1768.

XI. *A Letter from* John Ellis, *Esquire, F. R. S. to the President, on the Success of his Experiments for preserving Acorns for a whole Year without planting them, so as to be in a State fit for Vegetation, with a View to bring over some of the most valuable Seeds from the* East Indies, *to plant for the Benefit of our* American *Colonies.*

Read March 10, 1768.

HAVING discovered that the disappointment, which I met with about a year ago, in attempting to preserve through the season some ever-green oak acorns and some chesnuts in wax, was owing to their being unfit for vegetation at the time of my inclosing them; I resolved in my next attempt to try only such as I was persuaded were sound and fresh.

Fortunately, my curious and learned friend the Right Honorable Sir Thomas Sewell Master of the Rolls, hearing of my distress, offered to procure me some excellent acorns of the english oak, part of a parcel he had been sowing at his seat at Ottershaw near Chertsey in Surrey; these he was so obliging to send me the 20th of February 1767; part of them I sowed immediately under the windows of my chambers, in the kitchen garden of Grays Inn: and on the 22d of the same month I inclosed about 36 of them in bees-

 wax.

wax. Moſt of thoſe that I had ſown in the garden came up in June following 1767, and by the middle of September were 6 inches high.

This gave me ſome hopes that I ſhould not labour in vain as I had done before; for part of the ſame parcel of ever-green oak acorns which failed, I had given to Sir Thomas Sewell the year before to ſow, and he aſſures me that not one of them came up with him. I likewiſe diſcovered, though too late, that the Spaniſh cheſnuts, which failed, had been kiln-dried; this is a common practice in Spain, to prevent their ſprouting by the damp heat in the hold of the ſhip.

I ſhould not, my Lord, be ſo particular in explaining the cauſe of my diſappointment, but to ſhew the care that is neceſſary to be taken by perſons abroad in the choice of the ſeeds, as well as the ſtate they ought to be in, if they expect they ſhould anſwer the great end we propoſe.

Before I mention the method in which I treated theſe acorns, I muſt obſerve to your Lordſhip, that though I have formerly been ſo ſucceſsful as to preſerve both acorns and cheſnuts for the ſpace of a year in bees wax, ſeveral of which have afterwards vegetated, and ſome of them are now grown into trees; yet I always found that many of them were rotten when they were taken out of the wax; which made me ſuſpect that it was owing to the too great heat of the melted wax, that ſo many of them were deſtroyed. This put me on thinking of the following method to guard the ſeeds to be preſerved from too great heat, which I have now the pleaſure to ſhew your Lordſhip and the reſt of the Royal Society the good effects of.

After

After I had chofen out the faireft acorns, laying afide fuch as had fpecks proceeding from the wounds of infects, I wiped them very clean till they were quite bright, for fear of any condenfed perfpiration on the furface, which if inclofed, would turn to mouldinefs. I then poured fome melted bees-wax into a china plate about half an inch deep, and foon as the wax was cool, but ftill very pliable, I cut out with a penknife as much as would inclofe one acorn; this I wrapped round it, rolling it between my hands till the edges of the wax were perfectly united: in the fame manner I covered about 36 of them with all the caution in my power, fo that after they had been fet to harden I could not perceive the leaft crack in them. When they were quite cold and hard, I prepared an oval chip box, of 7 inches long, $4\frac{1}{2}$ broad, and $3\frac{1}{2}$ deep; into this I poured melted bees-wax to the depth of an inch and half; and when I could bear my finger in it, I laid the covered acorns at the bottom in rows as clofe as I could together; afterwards other rows over them, till the box was full; and when the firft wax began to cool, I poured fome wax that was barely fluid over the uppermoft acorns till they were quite covered. In order to cool them as foon as poffible, I fet the box near a window, where the fafh was raifed a little to let in a ftream of cold air; when they were almoft cold, I perceived the wax had fhrunk a little here and there, and left fome chinks; thefe I immediately filled up wich very foft wax, preffing it very clofe and fmooth. After it was quite cold and hard, I put on the cover of the box, and placed it on a fhelf in a clofet till the beginning of Auguft laft, when I fent it to the care of Mr. Dacofta, clerk to the Royal Society, to their houfe in Crane

Crane Court, to be produced and examined before the Royal Society at some of their first meetings after the long vacation. My health would not permit me to attend myself; but I am informed, my Lord, that when they were cut open and examined before your Lordship and the rest of the Royal Society present, their appearance promised success; and that they were ordered to be delivered by Dr. Morton, secretary to the Royal Society, to the care of Mr. William Aiton, Botanic Gardiner to her Royal Highness the Princess Dowager of Wales at Kew at my request, that the Royal Society might be informed whether they would vegetate.

I have just now, my Lord, had the pleasure of receiving a letter from Mr. Aiton, advising me, that he had sent to Mr. Robertson, housekeeper to the Royal Society, two pots with the young oaks rising from the acorns preserved in wax, which Dr. Morton sent him from the Royal Society in December last; and am well persuaded he has carefully attended to an experiment, the success of which, if properly followed, may in a few years put us in possession of the most rare and valuable seeds in a vegetating state from the remotest parts of the world, which in time may answer the great end of the improvement and advancement of our trade with our American Colonies. I am,

My Lord,

with the greatest respect,

Your Lordship's

most humble and obedient servant,

Gray's-Inn, March 9, 1768.

John Ellis.

SIR,

Kew, March 8, 1768.

S I R,

I Received December 5, 1767, of Doctor Morton of the British Museum a parcel of acorns preserved in wax (the quantity of acorns which I received was 34); and according to your desire and direction they were sowed, as soon as I received them, into a sandy light loom. I placed the pots with the acorns under a frame, where they remained till January the 28. I then took the pots with the acorns out of the frame, and placed them near a window, in one of our large airy stoves, where they have remained ever since; according to your desire, they shall be sent to-morrow to the Royal Society's house. I think the gentlemen of that Honourable Society will be pleased to see the method of preserving seeds in wax prove so successful; as the acorn is one of the worst of seeds to keep any time, out of the ground, from perishing; and the good success there is from those few which I received from Doctor Morton. I am, therefore, of the opinion, that, if seeds are sound and dry, and carefully put up in the wax, it is the best method that has ever been found out to preserve seeds from distant countries.

I am, Sir,

Your most obedient and humble servant,

William Aiton.

To John Ellis, Esq;
Gray's-Inn.

N. B. There are sixteen in one pot, and nine in the other, that are already come up, and most of them from four to six inches high.

XII. A

XII. *A Letter from Dr.* Donald Monro, *F. R. S. to* Mathew Maty, *M. D. Sec. R. S. inclosing one from Mr.* Farley, *of* Antigua, *on the good Effects of the Quassi Root in some Fevers.*

S I R,

Read March 17, 1768.

AS we have had no further accounts of the Quassi Root, since Dr. Linnæus published the sixth volume of his *Amœnitates Academicæ* * in the year 1764, I have, according to your desire, sent you the copy of a letter on the good effects of this root, which I hope will be acceptable to the Society, as it may excite Physicians to make trials of this medicine which seems to promise to be of so much use. The original letter was given me by the gentleman to whom it is addressed, while I attended him last year when he was here in England for the benefit of his health. I am,

S I R,

Your most obedient humble servant,

Jermyn-Street, March 8, 1768.

D. Monro.

* Dr. Carol. Linnæus gives a particular description and figure of the Quassi tree, which grows in the neighbourhood of Surinam, in South America, and of the Root having been administred at Surinam, with great success, in malignant, remitting and intermitting fevers: and he tells us that its virtues were first discovered by a slave of the name of Quassi, from whom the tree got its name.

Copy of a Letter from Mr. James Farley, *Practitioner in Physick in the Island of* Antigua, *to his Partner, Mr.* Arch. Glofter, *in* London; *dated* Antigua, July 26, 1767.

S I R,

MR. T—r has been extremely ill fince his arrival with a fever, which lafted for many hours; and, upon its going off, he could not retain the bark in any fhape whatever. Many things were tried to check the vomiting, and enable him to keep down fome bark, but to no purpofe. At laft I tryed the Quaffi Root, an account of which I read in one of the magazines for this year; it fat extremely well on his ftomach; he had no vomiting after the firft dofe, and recovered very fpeedily.

I have lately tried it in three or four cafes, where there has been a tendency to putrefaction and the bark would not ftay on the ftomach; a dram of this root, has effectually anfwered every purpofe that the bark would. It has this advantage over the bark, that it does not heat the patient.

I have given it in fevers, joined with the Radix Serpentariæ Virginianæ, with fuccefs. I had a pound or two from Efquebo, and have fent you a little of it.

Dr. Warner has fent Dr. Jackfon a piece of it; he faw the good effects of this medicine, in a patient, Captain B—n, who fails for London to-day. He attended him with me. I could not get the bark to fit

on his ſtomach, for he had a perpetual vomiting, and could not keep down any nouriſhment whatever. I prepared a decoction of a dram and a half of the Quaſſi Root, and a dram of the Rad. Serpent. Virgin. When it was ready, I ſent for Dr. Warner, that he might ſee the patient before I adminiſtred it; he complained of ſome pain on touching the pit of his ſtomach, had a very ſluggiſh low pulſe, a great pain over his eyes and in his eye balls, and vomitings. He took the decoction, which ſurprizingly put a ſtop to his vomiting; he had no return after the firſt doſe, and kept down every thing. We indeed gave him ſome Camphor and Sal Succini, on account of the ſluggiſhneſs of the pulſe, but I have tried it alone in a decoction, with infinite advantage.

Signed, J. Farley.

Received January 27, 1768. Read March 10, 1768.

XI. *Meteorological Observations for* 1767, *made at* Carlisle, Bridgwater, *and* Ludgvan; *and communicated by the Bishop of* Carlisle, *F. R. S.*

OBSERVATIONS made by Dr. CARLYLE at CARLISLE in CUMBERLAND.

N. B. Under column 1. stand the months. Column 2. contains an account of the baro[illegible] the highest and lowest morning and evening stations of the mercury, in the month, included wi[illegible] in he fa[illegible] [illegible] or more numbers are put before any station, it denotes it to have been alike on these several da[illegible]s. Colu[illegible] [illegible] account of the thermometer; noted in like manner as the barometer. Column 4. contains an ac[illegible]ount of the hygro[illegible] [illegible] first line notes the day of the month on which the greatest quantity of rain fell that month, and what that quanti[illegible] inches and decimals; the second the total depth of rain in the month. Column 5. contains the state of the wind &c.

Month.	Barometer.	Thermometer.	Hygrometer.		State of the &c.
January	Highest. 4. Morn. 30.1 4. Even. 30.125 Lowest. 11. Morn. 28.925 11. Even. 29.05	Highest. 31. Morn. 45 31. Even. 47 Lowest. 10. Morn. 25 10. Even. 25	22. Total	0.955 1.647	The month opened wit[illegible] very stormy winds from the North; succeded by frost, and some snow: but on the 10th began to fall the deepest and longest continued snow remembered here; it was measured to full 14 inches deep, on the level, in Carlisle, in the evening of the 11th; and the ground remained covered thus with it till the 26th and 27th; while the first snow fell, the wind was North; when the great one fell it was South-West, and very high. The remainder of the month it was chiefly southerly. Though the snow went off remarkably gently, yet it occasioned two very great floods, on the 26th and [illegible]7th; till which time, the thermometer had kept mostly c[illegible]nsiderably under 40. The 10th it was the lowest fo[illegible] the whole day I ever remember it.

February

	Baromet	ter.	Hygrometer.		State of the Winds, Weather, &c.
February	Highe 25. Morn. 29 95 3 24. Even. 29.95 Loweft. 14. Mo 13 26. Ev	Higheft. 1, 2, 4, 5. Morn. 47 2, 3, 4, 11. Even. 48 Loweft. 1, 19, 21, 28. Morn. 43 28. Even. 43	24. Total	0.41 2.426	Weather pretty mild and open, but cloudy, with frequent rains. A flood on the 17th. Wind till the 8th, from the South; then northerly nigh a week; afterwards moftly in the fouthern points. On the 20th a ftorm from South-Weft.
March	Higheft. 30. Morn. 30.125 9. Even. 30.125 Loweft. 19. Morn. 29.075 18. Even. 29.2	heft. 31. Morn. 50 31. Even. 51 Loweft. 15. Morn. 35 14. Even. 38	18. Total	0.366 1.586	Weather cloudy, with fmall rain till the 6th; when it became fair and frofty till the 15th; the middle of the month was very wet, and fnow fell on the hills: the latter end was mild and fair. Winds moftly in the Weft quarter till the laft week, when it turned eafterly. The barometer was in general high.
April	Higheft. 30. Morn. 30.45 30. Even. 30.475 Loweft. 22. Morn. 29.6 22. Even. 29.7	Higheft. 10, 14. Morn. 52 30. Even. 55 Loweft. 17, 18. Morn. 43 6, 17, 18. Even. 46	1. Total	0.135 0.211	Barometer in general very high through this month; the weather fair and dry. On the 16th and 17th very hard froft; on the 18th ice one-tenth of an inch thick on the roads; on the 19th all the hills covered with fnow. Wind in the beginning from South and Eaft; afterwards often from the North and Eaft.
May	Higheft. 1. Morn. 30.475 1. Even. 30.4 Loweft. 30. Morn. 29.225 29, 30. Even. 29.3	igheft. 3. Morn. 58 18. Even. 60 Loweft. 6. Morn. 47 5, 7. Even. 49	30. Total	0.908 3.141	Barometer lower in general than laft month. Weather favourable till the 9th; rainy and moift till the 13th; dry and dufty till the 24th; after this rainy, till the end. Wind in the middle of the month North and Eaft. Its beginning and end South and Weft. Cuckow heard on the 12th.
June	Higheft. 9, 10. Morn. 30.4 9. Even. 30.4 Loweft. 1. Morn. 29.575 1. Even. 29.575	igheft. 11. Morn. 61 9, 11. Even. 62 veft. 5. M 3. Ev	24. Total	0.174 0.559	Weather to the 14th bright and fair. Winds North or Eaft. After that frequent fhowers, to the end of the month, from the North-Weft or South-Weft quarters.

Month.	Barometer.	Thermometer.	Hygrometer.		State of the Winds, Weather, &c.
July	Higheft. 17. Morn. 30.05 16. Even. 30.05 Loweft. 4. Morn. 29.25 3. Even. 29.15	Higheft. 20. Morn. 62 19, 20, 30. Even. 62 Loweft. 2, 6, 16. Morn. 56 1. Even. 53	6. Total	0.414 5.941	Very few days quite fair this month, yet the rain fell fo much in fhowers, or in the night, the hay harveft was not much interrupted by it. Wind moftly from the South or Weft points. Thunder on the 12th and 20th, with violent rains to the Eaft of us. On the 23d great lightnings.
Auguft	Higheft. 27, 28. Morn. 30.2 27. Even. 30.225 Loweft. 15. Morn. 29.375 14. Even. 29.35	Higheft. 5. Morn. 67 4, 29. Even. 68 Loweft. 19. Morn. 55 19. Even. 55	14. Total	0.296 2.03	Weather very variable, as were the winds, with frequent and fudden changes of fhowers and funfhine; towards the end of the month more fettled weather. Barometer in general low. Thunder, with fevere fhowers, on the 4th. N. B. The thermometer at Rofe Caftle, in the open air, hung againft the North wall of the tower, on the 4th rofe to 80 at noon; at 1 p. m. was at 78; at 2, 77; at 3, 76.
Septemb.	Higheft. 21. Morn. 30.45 21. Even. 30.45 Loweft. 7. Morn. 29.55 4, 6. Even. 29.575	Higheft. 21. Morn. 65 21, 24. Even. 64 Loweft. 30. Morn. 54 30. Even. 54	14. Total	0.552 3.065	The wind moftly foutherly. On the 6th thunder with fevere fhowers. From the 16th to the 26th bright clear weather, without a fhower. Some bluftering winds from the North-Weft in the end of the month.
October	Higheft. 13, 14. Morn. 30.35 12, 13. Even. 30.35 Loweft. 4. Morn. 29.0 4. Even. 29.15	Higheft. 26. Morn. 58 24, 26. Even. 57 Loweft. 9, 11. Morn. 43 8. Even. 45	7. Total	0.572 2.954	Wind moftly in the Weft. The 3d and 4th very ftormy from the Weft and North-Weft. On the 8th a flood, and the hills covered with fnow. The 9th and 10th hard froft, with thick ice upon the waters; which was fucceeded by rain. From the 18th were high winds from the South-Weft for nigh a week. The 29th, 30th, and 31ft, were alfo very ftormy from the fame quarter.

November

Month.	Barometer.	Thermometer.	Hygrometer.	State of the Winds, Weather, &c.
Novemb.	Higheſt. 28. Morn. 30.3 23. Even. 30.225 Loweſt. 14. Morn. 28.65 14. Even. 28.95	Higheſt. 10. Morn. 55 9. Even. 54 Loweſt. 16. Morn. 38 15. Even. 41	14. 0 593 Total 4.084	Winds moſtly from the South or Weſt, with almoſt conſtant rains till the 16th; inſomuch that we had a remarkably high flood on the 7th; and three other very conſiderable ones on the 10th, 12th, and 14th. On the 1[illegible]th the hills all round were covered with ſnow. We had very ſtormy winds on the 10th, 11th, and 13th. On the 14th hail, thunder, and lightning: the lightnings had been very frequent in the nights, particularly from the 8th, and continued to the 22d, though thunder was rarely heard. On the 15th a ſmart froſt ſet in for three days. On the 19th was another, but leſs flood; from thence cloudy and ſhowery to the end of the month. On the 26th coughs were almoſt univerſal among the horſes, both in town and country; though very few died.
Decemb.	Higheſt. 3. Morn. 30.575 2. Even. 30.55 Loweſt. 7, 8. Morn. 29.45 7. Even. 29.45	Higheſt. 16. Morn. 50 15. Even. 49 Loweſt. 31. Morn. 26 30. Even. 27	14. 0.203 Total 0.624 Tot. for the Y. 26.268	Winds in the Weſt to the 16th. Weather cloudy and ſhowery, with ſnow on the hills. From the 16th to the end bright, with froſt. Winds from the Eaſt. Snow began to fall on the 24th; on the 27th it fell 2 inches and a quarter deep; which, coming with hard froſt, remained on the ground to the end of the month.

G. Carlyle.

OBSERVATIONS

OBSERVATIONS at BRIDGWAT So ERSE

Month.		Barometer.	In.	Dec.	State of the Weather and Wind.		Fahrenheit's Thermometer.	Deg.	Ombrometer. In.	Dec.
January	1. 14.	Higheſt Loweſt	30 28	,23 ,85	The firſt part of the month froſty. Wind N. E. and N. W. The latter part ſtormy, with high winds, and variable from N. W. to S. W. The 11th the ſnow was 6 inches deep, undriven.	31. 10.	Higheſt Loweſt	52 15	1	,265
February	24. 20.	Higheſt Loweſt	30 29		Stormy throughout. The wind in general either S. W. or S. E.; and in the latter part of the month high.	26. 23.	Higheſt Loweſt	51 36	3	,372 $\frac{1}{2}$
March	6. 19.	Higheſt Loweſt	30 29	,15 ,08	The firſt part cloudy and ſunſhine alternately. The latter part intermixed with ſtorms. The wind generally S. W. in the firſt part; and variable to N. W. and S. E. in the latter.	31. 15.	Higheſt Loweſt	51 34	1	,802 $\frac{1}{2}$
April	30. 21.	Higheſt Loweſt	30 29	,45 ,25	The firſt and laſt parts ſettled, with ſunſhine and cloudy intermixed. From the 20th to the 26th ſome rain. The wind generally N. E.	1. 17.	Higheſt Loweſt	53 37	1	,217 $\frac{1}{2}$
May	1. 30.	Higheſt Loweſt	30 29	,40 ,18	The firſt part ſettled fair. The middle ſtormy, and the latter part changeable. The wind for much the greater part N. W.	12. 5.	Higheſt Loweſt	59 41	1	,147 $\frac{1}{2}$
June	8. 3.	Higheſt Loweſt	30 29	,29 ,39	The firſt week ſtormy. The remainder ſettled fair. The wind N. W. or N. E.	25. 3.	Higheſt Loweſt	67 43		,920
July	27. 5.	Higheſt Loweſt	29 29	,94 ,28	Unſettled, and rainy in general. The wind almoſt conſtantly S. W.	29. 5.	Higheſt Loweſt	67 53	4	,107 $\frac{1}{2}$

August

Month.		Barometer.	In.	Dec	State of the Weather and Wind.		Fahrenheit's Thermometer.	Deg.	Ombrometer. In.	Dec.
August	27.	Highest	30	,15	The first week settled fair. The remainder unsettled and rainy. The wind generally N. W. and gentle.	4.	Highest	66	2	,870
	14.	Lowest	29	,44		17.	Lowest	52		
September	21.	Highest	30	,32	The first part changeable and stormy. The latter part settled fair. The wind variable. The first and latter part rather strong; in the middle gentle.	4.	Highest	64	2	,245
	4.	Lowest	29	,50		29.	Lowest	52		
October	13.	Highest	30	,34	From the 1st to the 9th windy and stormy. Settled to the 17th; and from thence to the end unsettled. The first part the wind generally N. E. and N. W. The latter part S. W.	26.	Highest	65	3	,572 ½
	3.	Lowest	29	,18		13.	Lowest	36		
November	28.	Highest	30	,37	The first part unsettled, with high winds. The latter part cloudy and foggy, with gentle wind. In the beginning and latter part the wind N. W.; and in the middle generally S. W.	9.	Highest	59	1	,932 ½
	14.	Lowest	28	,08		17.	Lowest	31		
December	3.	Highest	30	,57	To the 22d generally cloudy; and from thence to the end frosty. The wind through the whole month gentle, and generally either N. E. or N. W. At 10 A. M. On the 3d of this month the mercury in the barometer stood at 30 ,59.	9.	Highest	52		,477 ½
	19.	Lowest	29	,20		31.	Lowest	17		
							Total of Rain		24	,850

OBSERVATIONS

OBSERVATIONS at LUDGVAN, in MOUNT BAY CORNWALL.

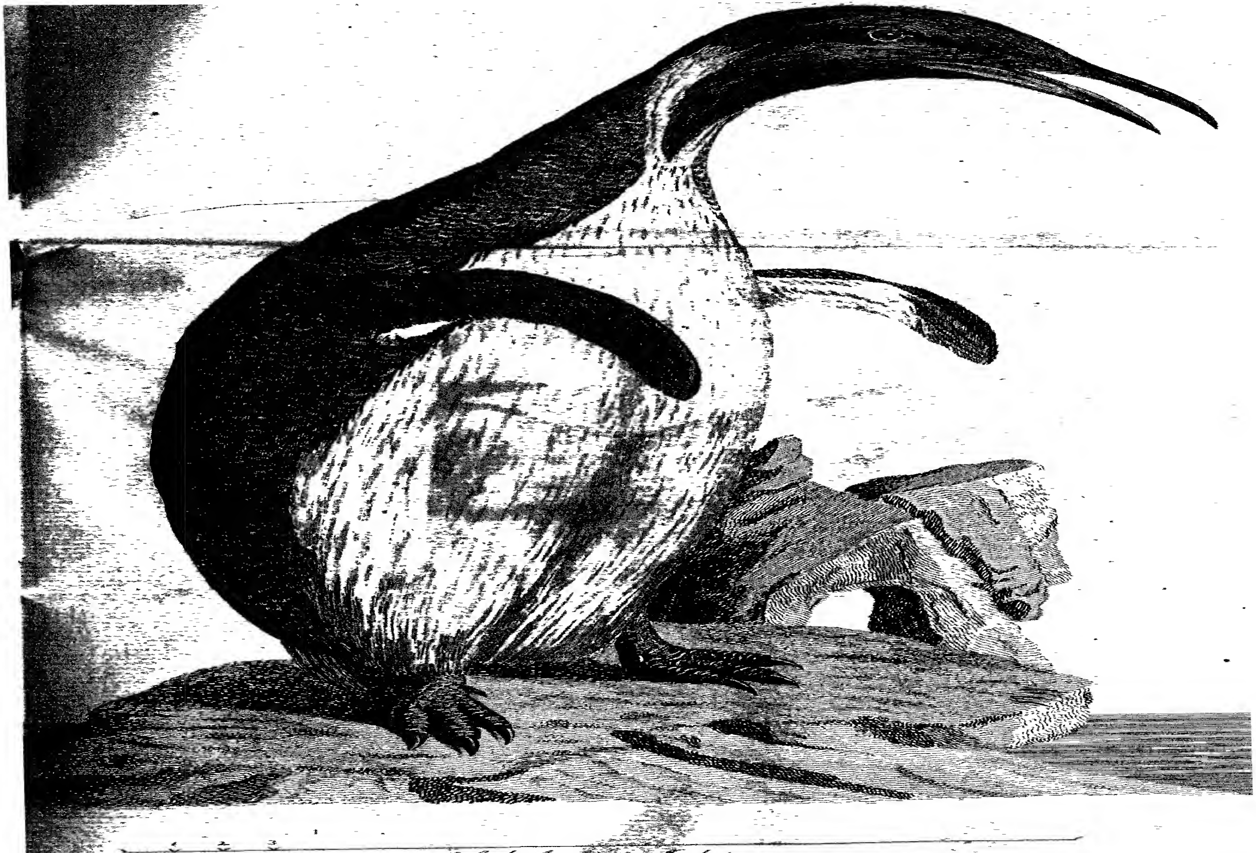
1767. Month.	Barometer. Nonius's Division.			General State of the Weather and Wind.	Fahrenheit's Therm. high. low. and Med. of three Obs. each Day; the Therm. constantly exposed in the Air, never to the Sun.				Ombr.
		In.	Dec.					Med.	In.
January	1. Highest 13. Lowest	30 28	,17 ,50	Colder than usual. On the 8th snow began to lie upon hard frost. On the 11th snow near 8 inches deep in plains. On the 13th a quick thaw; and on the 14th 8 a. m. snow and frost all gone. Wind mostly N. and E. Stormy.	27. 12.	Highest Lowest	48 31	41 $\frac{6}{31}$	3 725
February	24. Highest 20. Lowest	29 28	,91 ,89	Misty, stormy, rainy. On the 12th a storm from midnight to 4 a. m. On the 20th a violent storm, with much rain. Wind mostly W.	7. 23.	Highest Lowest	49 42	46 $\frac{12}{28}$	6 370
March	5. Highest 27. Lowest	30 29	,8 ,7	Cloudy. Calms. In the middle, for two days, sleet, calms, and frost; rest variable, calm, wind, and rain. Winds variable.	31. 16.	Highest Lowest	50 39	45 $\frac{1}{31}$	3 600
April	30. Highest 23. Lowest	30 29	,26 ,5	Misty. Fair. Calms. Winds mostly N. E. and W. nearly equal.	26. 19.	Highest Lowest	51 43	47 $\frac{2}{30}$	2 000
May	1. Highest 31. Lowest	30 29	,27 ,15	Variable.	26. 6.	Highest Lowest	57 47	53 $\frac{6}{31}$	1 900
June	8. Highest 2. Lowest	30 29	,20 ,37	Variable; but mostly mists and calms. Winds generally had some point of the N.	25. 4.	Highest Lowest	67 46	57 $\frac{4}{30}$	0 500
July	1. Highest 8. Lowest	29 29	,90 ,34	Frequent rain and showers. Wind mostly W.	22. 5.	Highest Lowest	$62\frac{1}{2}$ 50	57 $\frac{1}{31}$	5 775

Month.	Barometer. Nonius's Diviſion.				State of the Weather and Wind.	Fahrenheit's Thermometer. Higheſt, Loweſt, and Medium, of three Obſerv. each Day.				Ombr.	
			In.	Dec.					Med.	In.	
Auguſt	26. 20.	Higheſt Loweſt	30 29	,10 ,45	Calm. Sunſhine. Showery. Winds variable, nearly equal.	28. 26.	Higheſt Loweſt	67 52	$58\frac{17}{31}$	2	200
September	20. 4.	Higheſt Loweſt	30 29	,24 ,37	Variable Winds; the Weſt prevailing. Miſty calms, fogs, with ſunſhine mornings.	20. 8.	Higheſt Loweſt	66 51	$57\frac{21}{30}$	2	390
October	13. 4.	Higheſt Loweſt	30 29	,13 ,20	Rainy. Stormy. Calms. Windy; moſtly from the Weſt.	2. 12.	Higheſt Loweſt	59 41	$52\frac{1}{31}$	4	140
November	28. 16.	Higheſt Loweſt	30 28	,26 ,93	Miſty ſhowers and rain. Stormy. Violent Storm on the 13th from S. S. W. Thunder and lightning, with ſhowers; the extreme at 11 p. m. Wind moſtly Weſt.	3. 16.	Higheſt Loweſt	57 40	$50\frac{4}{30}$	2	580
December	3. 19.	Higheſt Loweſt	30 28	,42 ,80	Calm miſts. In the middle of the month ſtormy with rain. On the 19th a ſtorm. On the 20th ditto; reſt hard froſt with ſome ſnow.	10. 26.	Higheſt Loweſt	53 29	$40\frac{17}{31}$	1	920

Total of Rain this Year at Ludgvan $37\frac{100}{1000}$

William Borlaſe.

XIV. *Account*



A Scale of [illegible] Inches.

XIV. *Account of the different Species of the Birds, called* Pinguins, *by* Thomas Pennant, *Esquire, F. R. S.*

PINGUINS.

Read March 17, 1768.

THE characters of this genus are, very small wings and those covered with meer shafts. Four toes on each foot, three of which are webbed, the fourth loose and standing forward.

1. The Patagonian Pinguin.

Tab. V.

Size. The length of the stuffed skin, we measured, was four feet three inches; and the bulk of the body seemed superior to that of a swan.

Bill. Four inches and a half long; slender, strait, bending only on the end of the upper mandible, black, covered on each side the base with soft short brown feathers; the sides of the lower mandible compressed, the lower part or base orange coloured, the end dusky. No nostrils.

Tongue. Half the length of the bill, and singularly armed with strong sharp spikes pointed backwards.

Plumage. The moſt remarkable of all the feathered tribe, each feather lying over the other, with the compactneſs of the ſcales of fiſh; their texture is equally extraordinary; the ſhafts broad and very thin; the vanes unwebbed; the head, throat, and hind-part of the neck, are of a deep brown colour; from each ſide of the head to the middle of the fore-part of the neck are two lines of bright yellow, broad above, narrow beneath, and uniting half way down; from thence the ſame colour widens towards the breaſt, fading away till it is loſt in pure white, of which colour is the whole under ſide of the body, a duſky line dividing it from the colour of the upper part; the whole back is of a very deep cinereous colour, almoſt duſky; but the end of each feather is marked with a cœrulean ſpot, thoſe about the junction of the wings larger and paler than the others.

Wings. Are extremely ſhort in reſpect to the bulk of the bird, hang down, and have rather the appearance of fins, whoſe office they perform *; their length is only fourteen inches; on the outſide they are duſky, and covered with ſcale-like feathers, or at beſt with ſuch whoſe ſhafts

* De Veert's Voyage, p. 333. Winter's Voyage in Hacluyt's Coll. III. 752.

are

are ſo broad and flat as ſcarce to be diſtinguiſhed from ſcales; thoſe on the ridge of the wings conſiſting entirely of ſhaft; the larger or quill feathers have ſome very ſhort webs.

Tail. Conſiſts of thirty brown feathers, or rather thin ſhafts, reſembling ſplit whale-bone, flat on their upper ſide, concave on the under, and the webs ſhort, unconnected, briſtly.

Legs and Feet. From the knees to the end of the claws ſix inches, covered with ſtrong pentangular black ſcales; the fore-toe ſcarce an inch long, and the others ſo remarkably ſhort, as to evince the neceſſity of that ſtrength of the tail, which ſeems intended as a ſupport to the bird in its erect attitude; in the ſame manner as that of the wood-pecker is when it clings to the ſides of trees; between the toes is a ſtrong ſemilunar membrane, continued even up part of the claws; the middle claw is near an inch long, and the inner edge very ſharp and thin; the interior toe is ſmall, and placed very high.

Skin. Extremely tough and thick, which, with the cloſeneſs of the feathers, guards it effectually in the element it is ſo converſant in.

Hiſtory. This bird was brought by Capt. MacBride, from the Falkland Iſles, off the Straits of Magellan; we believe this ſpecies

ſpecies to have been undeſcribed; for the birds that bear the ſame name are mentioned by every writer, who treat of them as far inferior in ſize to this; ſome compare their bulk to that of a duck; but none make it larger than a gooſe; the colours alſo of this ſpecies are too ſtriking not to have been taken notice of, had it been before diſcovered.

Captain Mac-Bride was ſo obliging as to inform us that this was a very ſcarce ſpecies; though he ſaw in the ſame place multitudes of the leſſer kind, with which it agreed in its manner of life. Since the natural hiſtory of each ſpecies is the ſame, we ſhall give a general view of the œconomy, &c. from ſuch writers who have treated of them.

It is agreed that they are inhabitants of ſouthern latitudes only, being, as far as is yet known, found only on the coaſts of South America, from Port Deſiré to the Straits of Magellan; and, if we remember right, Frezier ſays, they are found on the weſtern ſhore, as high as Conception. In Africa they ſeem to be unknown, except on a ſmall iſle near the Cape of Good Hope, which takes its name from them.

They are found in prodigious numbers on land during the breeding ſeaſon; for they ſeldom come aſhore but at that

time; they form burrows under ground, like rabbits; and the iſles they frequent are perfectly undermined by them, ſo that it is difficult to walk without falling into their holes, or ſinking through the ſurface up the ſhoulders. Such rencontres are diſagreeable, as theſe birds bite extremely hard; and commonly three or four are found to neſtle together in the ſame hole.

Their eggs are ſaid to be rather leſs than that of a gooſe; and that they begin to lay the latter end of September, or beginning of October.

Their attitude on land is quite erect; and on that account they have been compared by ſome to pygmies, by others to children with white bibs *.

On land they are exceſſively aukward, by reaſon of the ſituation of their legs, which are placed quite behind: they are very tame, and may be drove like a flock of ſheep; when preſſed, they ſeek for ſhelter either in their burrows, or the ſea, which ſeems to be their more natural element.

In the water they are remarkably active, and ſwim with vaſt ſtrength, aſſiſting by their wings, which ſerve inſtead of fins.

Their food in general is fiſh, not but that they will eat graſs like geeſe; for Sir

* Narborough's voyage, p. 59.

Richard

Richard Hawkins obſerved, in an iſle they frequent off Patagonia, a ſmall valley covered with graſs, which the birds never burrowed in, as if they meant to reſerve it for paſturage.

They are very fat, but taſte very fiſhy, not unlike our puffins: as they are very full of blood, it is neceſſary to cut off their heads as ſoon as they are killed, in order that it may run out; it is alſo requiſite they ſhould be flayed, for without thoſe precautions their fleſh is ſcarce eatable. When ſalted it becomes a good food, as navigators have often experienced, in particular Richard Hopkins *, who preſerved that way ſixteen hogſheads, which laſted above two months, and ſerved as beef.

Theſe birds and ſeals ſeem to have been beſtowed in quantity on thoſe deſolate ſhores, as reſources in extremity to diſtreſſed voyagers.

Name. The proper name of theſe birds is Pinguin *(propter pinguedinem †)*, on account of their fatneſs. It has been corrupted to Penguin; ſo that ſome, imagining it to have been a Welſh word ſignifying a white head, entertained ſome hopes of tracing the Britiſh colony, ſaid to have migrated into America, under the auſpices of Madoc Gwineth, ſon of Owen

* Sir Richard Hawkins, Obſ. 72.
† Cluſ. Exot. 101.

Gwineth,

Gwineth, A. D. 1170 *. But as the two ſpecies of birds that frequent that coaſt have black heads, we muſt reſign every hope founded on that hypotheſis of retrieving the Cambrian race in the new world.

We give this ſpecies the epithet Patagonian, not only becauſe it is found on that coaſt, but becauſe it as much exceeds in bulk the common kinds, as the natives are ſaid to do the common race of men.

I muſt not quit this ſubject without making my acknowledgements to Mr. Banks for communicating this curious bird to me, which he now permits to be laid before the Society for their examination.

II. The leſſer PINGUIN.

Anſer Magellanicus. Cluſ. Exot. 101.
Black-footed Penguin. Edw. 94.
Diomcdea demerſa. Linn. Syſt. 214.

Size. Of a gooſe.

Bill. Strait to the point, where it grows hooked; the end of the lower mandible abrupt, as if cut off; both are black, but marked acroſs near the ends with a yellow bar.

lumage. The crown, hind-part of the head, the cheeks, and chin, are duſky; from the

* Powel, Hiſt. Wales, p. 229.

 bill

bill over the eyes, and then down to the neck, passes a white line; the back, and outside of the wings, are of the same colour with the head; the breast, belly, and sides, are white, marked with a brown line passing over the upper part of the breast, under the wings, and terminating at the legs.

They vary in colour; some wanting the white line over the eye, and the brown one over the breast.

Wings. Like those of the preceding.

Legs. Black, which agrees with the Magellanic goose of Clusius, and may be the lesser species observed by our late voyagers to the coast of Patagonia.

III. The red-footed PINGUIN.

The Penguin. Edw. 49.
Le Gourfou Calaractes. Brisson. Av. VI. 102.
Phaeton demersus. Lynn. Syst. 219.

Size. Inferior to the last.

Bill. Thick, arched, and red.

Plumage. Like in texture to that of the former; the head, hind part of the neck, and the back, of dusky purplish hue; breast and belly white.

Wings. Brown, but the tips of the larger feathers white.

Tail. None, in lieu of it a few black bristles.

Legs. Red.

History.

History. This ſeems to be the African ſpecies; for all that have deſcribed the South American kinds attribute to them black legs.

This is found on Penguin iſle, near the Cape of Good Hope, of which Sir Thomas Roe, in his Voyage to India, gives this brief relation:

"On the iſle of Penguin is a ſort of fowl "of that name, that goes upright; his "wings without feathers, hanging down "like ſleeves faced with white; they "do not fly, but walk in parcels, "keeping regularly their own quar-"ters *."

Leſt the bird known, by the name of Penguin, in the northern parts of Europe and America, ſhould be confounded with theſe, it may be obſerved, that it is of another genus; and it is by the later ornithologiſts very juſtly ranked with the Auks.

* In Churchill's Coll. of Voyage, vol. I, p. 767.

XV. *The Application of Dr.* Saunderſon's *Theorem for ſolving unlimited Equations, to a* curious Queſtion in CHRONOLOGY: *By Mr.* James Horſefall, *F. R. S.*

Read March 24, 1768.

BY old tables it appears that Eaſter day happened on the 22d of March (which is the *ſooneſt* it ever can happen), in the years of Chriſt 165, 697, 1229, and laſtly in 1761.

Queſt. 1. What is the next year of our Lord, when it will happen ſo again before 1900? For,

Note. From thence to 2199, the paſchal full moon, or the golden number 14, which diſtinguiſhed the years above, will be fixed *on* the 22d of March; *conſequently* EASTER *day cannot happen before the* 23*d of March in that period.*

Anſwer. In the act for altering the ſtile, it appears by the table for finding Eaſter till 1899, that this can *never* happen in *that* period, but when the *golden number*, or lunar cycle, is 14, and the *Sunday* letter D.

Alſo, by making a *ſolar cycle* for that century, the firſt year of it will fall on 1812, the Sunday letters E D, wherefore all the years in that cycle, which have D for the Sunday letter, are 1, 7, 18, 24: and now the queſtion is reduced to this.

Queſt.

Queft. What year of our Lord in the 19th century will have the folar cycle either 1, 7, 18, or 24, *when* the lunar cycle is 14? Or, which is the fame thing;

Queft. What number is there between 1800 and 1900, which divided by 28 leaves 1, 7, 18, or 24; and being divided by 19 leaves 14?

SOLUTION.

Here then in the general theorem $\frac{ar}{l} \times d - e + d$, is $a = 28$, $b = 19$, $D = 1, 7, 18$, or 24; $E = 14$, $l = 1$.

To find r, the quotients are * 1, 2, 9; drop the firft and laft, becaufe their number is odd: then the feries required will be 0, 1 2; therefore $r = 2$.

* 19)28(1
9)19(2
1)9(9

N. B. If to any year of Chrift be added 9, and the fum divided by 28; the remainder, or 28, if 0 remains, will be the *cycle of the fun* for that year; and if 1 be added to any year of Chrift, and the fum divided by 19; the remainder, or 19, if 0 remains, is the *cycle of the moon*. Hence, if any year of Chrift be feverally divided by 28 and 19, and the remainders be d and e refpectively; then $d + 9$, or $d + 9 - 28 = D$; and $e + 1$, or $e + 1 - 19 = E$. In the prefent cafe, taking $D = 1$, $d + 9 = 1$ cannot be; becaufe d would be *negative*: but d muft *not only be affirmative*, but alfo GREATER than e; wherefore make $d + 9 - 28 = D = 1$;

therefore

therefore $d = 1 + 28 - 9 = 25$; and $e + 1 = E = 14$, therefore $e = 13$.

D and E are the *cyclar* numbers, and d and e are the *anno domini* numbers ſuited to the theorem.

Here then $\overline{ar \times d - e} + d = \overline{28 \times 2 \times 7} + 20 = 412$; and $412 + 532 + 532 + 532 = 2008$. The firſt anſwer, therefore, A. D. 412, is too little, and when encreaſed by three *dionyſian* periods, or *multiples* of 28 and 19 is too big, going beyond the century required. So, *when this ſolar cycle is* 1, *it will not do.*

Let D = 7, the reſt as before. Then $d + 9 - 28 = 7$; therefore $d = 26$. Here then $\overline{ra \times d - e} + d = \overline{28 \times 2 \times 13} + 26 = 754$; and $754 + 532 + 532 = 1818$. So A. D. 1818 WILL ANSWER THE QUESTION.

Let D = 18, the reſt as before. Then $d + 9 - 28 = 11$; therefore $d = 37$. Here $\overline{ra \times d - e} + d = \overline{28 \times 2 \times 24} + 37 = 1381$; and $1381 + 532 = 1913$. This goes beyond the century required; ſo *will not do.*

Let D = 24, the reſt as before. Then $d + 9 = 24$; therefore $d = 15$. Here $\overline{ra \times d - e} + d = \overline{28 \times 2 \times 2} + 15 = 127$; $127 + 532 \times 4 = 2255$; which goes *beyond* the century required.

So there is *but one year* in the 19th century, *viz.* 1818, that will have the conditions required. The cycle of the ſun will then be 7; the cycle of the moon 14; and the Sunday letter D; and *Eaſter Day the 22d of March*

N. B. For

N. B. For *every century* a *new* ſolar cycle muſt be made; becauſe, by the Act of Parliament* for correcting the calendar, every 100th year for three centuries is *common*, and *not biſſextile*; ſo that the ſame dominical letter ſtands againſt the ſame year, in the cycle only for 100 years in *three* ſucceſſive centuries.

N. B. By a continual addition of 28 to 1700 or 1756 †, we have the *firſt* year of each ſolar cycle; and when the *firſt* year of that cycle *next after the beginning* of any century is had, and its dominical letter found, by the rules and tables in the act, *the cycle for that century may be formed*, with the dominical letters anſwering to each year of it; whereby may be ſeen on *what* years of the cycle *the ſame Sunday letter recurs*. Thus;

Queſt. 2. If it was required to find in what years between 2200 and 2300 Eaſter Day would again happen on 22d of March; I find by the hints above, that the *firſt* year of the ſolar cycle falls on 2204; and, being *leap-year*, I find by the rules and tables in the act, that the *dominical-letters* are A G: from thence I conſtruct the ſolar cycle of 28 years, as in table I.

And from the table prefixed to the late Earl of Macclesfield's Letter to Martin Folkes, Eſq; P. R. S. read May 10, 1750, and publiſhed in Phil. Tranſ. Vol. XLVI. p. 47. ſhewing the place of the golden

* 24 George II.
† Vide Table I.

numbers in the calendar, and the paſchal full moon, and the Sunday letter, anſwering thereto for that century (which ſtand as in table II), I conſtruct table III, for finding Eaſter Day during *that* century; and obſerve it *never* happens on the 22d of March, but when the *golden number* C, and the *dominical letter* D.

And the dominical letter D happens only in the 4th, 9th, 15th, and 26th years of the ſolar cycle in that century, as appears by table I.

Now the queſtion is reduced to this, *viz.*

What number is there between 2200 and 2300; which, being divided by 28, leaves either 4, 9, 15, or 26; and being alſo divided by 19, leaves 6?

SOLUTION.

In the general theorem above, *viz.* $\frac{ar}{l} \times d - e + d$ are given $a = 28$, $l = 1$, $r = 2$, as before; and to find the values of d and e,

We have $d + 9 - 28 = D = 4$; therefore $d = 23$ }
And becauſe $e + 1 = E = 6$; therefore $e = 5$ } in the theorem:

viz. $\overline{28 \times 2 \times 18} + 23 = 1031$; and $1031 + 532 + 532 = 2095$: ſo that this cyclar number will *not* do, the year falling either below or beyond the century required.

2. Let $D = 9$; the reſt as before. Then ſince $d + 9 - 28 = 9$; therefore $d = 28$, and $e = 5$ as before; and $\overline{28 \times 2 \times 23} + 28 = 1316$; and $1316 + 532 + 532 = 2380$. This cyclar number will *not* do, for the ſame reaſon as the laſt.

3. Let

3. Let $D = 15$; the reft as before. Then fince $d + 9 = D = 15$; therefore $d = 6$, and $e = 5$, as before; and $\overline{28 \times 2 \times 1} + 6 = 62$; and $62 + \overline{532 \times 4} = 2190$. This cyclar number will *not* do, for the fame reafon as before.

4. Let $D = 26$; the reft as before. Then $d + 9 = 26$; therefore $d = 17$, and $e = 5$, as before; and $\overline{28 \times 2 \times 12} + 17 = 682$; and $689 + \overline{532 \times 3} = 2285$; and this is the *only* year that will anfwer the queftion; becaufe it has 6 for its golden number, and D for its dominical letter. Whence we may conclude, that after A. D. 1761, there will not be fo long a Trinity-vacation again till 1818; and after that year, the like will not happen till 2285.

Table I.
Solar Cycle, &c. for the 23d Century.

1756
112 = 4 × 28
1868
112
1980
112
2092
112

	Dominical Letters.	Years of the Sol. Cyc.
2204	A G	1
5	F	2
6	E	3
7	D	4
8	C B	5
9	A	6
10	G	7
11	F	8
12	E D	9
13	C	10
14	B	11
15	A	12
16	G F	13
17	E	14
18	D	15
19	C	16
20	B A	17
21	G	18
22	F	19
23	E	20
24	D C	21
25	B	22
26	A	23
27	G	24
28	F E	25
29	D	26
30	C	27
31	B	28
32	A G	1

Table III.
For finding Easter from 2200 to 2299, by the Golden Number and Dominical Letters.

G. N°	A	B	C	D	E	F	G
I	Apr. 16	17	18	19	20	21	22
II	Apr. 9	10	11	5	6	7	8
III	Mar. 26	27	28	29	30	31	25
IV	Apr. 16	17	18	19	13	14	15
V	Apr. 2	3	4	5	6	7	8
VI	Mar. 26	27	28	22	23	24	25
VII	Apr. 16	10	11	12	13	14	15
VIII	Apr. 2	3	4	5	Mar. 30	31	Apr. 1
IX	Apr. 23	24	18	19	20	21	22
X	Apr. 9	10	11	12	13	7	8
XI	Apr. 2	Mar. 27	28	29	30	31	Apr. 1
XII	Apr. 16	17	18	19	20	21	15
XIII	Apr. 9	10	4	5	6	7	8
XIV	Mar. 26	27	28	29	30	24	25
XV	Apr. 16	17	18	12	13	14	15
XVI	Apr. 2	3	4	5	6	7	1
XVII	Apr. 23	24	25	19	20	21	22
XVIII	Apr. 9	10	11	12	13	14	15
XIX	Apr. 2	3	4	Mar. 29	30	31	Apr. 1

Table II.
Lunar Cycle, &c. for the 23d Century.

Gold. Numbers.	D. of the Month for Paschal Full Moons.	Domin. Let.
6	Mar. 21	C
	22	D
14	23	E
3	24	F
	25	G
11	26	A
	27	B
19	28	C
8	29	D
	30	E
16	31	F
5	Apr. 1	G
	2	A
13	3	B
2	4	C
	5	D
10	6	E
	7	F
18	8	G
7	9	A
	10	B
15	11	C
4	12	D
	13	E
12	14	F
1	15	G
	16	A
9	17	B
17	18	C
	19	D
	20	E
	21	F
	22	G
	23	A
	24	B
	25	C

XVI. *A Determination of the Solar Parallax attempted, by a peculiar Method, from the Observations of the last Transit of Venus: By* Andrew Planman, *Professor of Natural Philosophy, in the University of* Aböa, *and Member of the Academy of Sciences at* Stockholm; *together with a Letter from him to Mr.* James Short, *F. R. S.*

Vir Celeberrime;

Read March 24, 1768.

DUM ante binos annos, in Transactionibus Philos. anni 1763, quæ in Bibliotheca Regiæ Academiæ Scientiarum Stockholmensis servantur, animadverti te, vir celeberrime, exquisitissimam collocasse operam in investiganda parallaxi solis; mihi proposui, mearum lucubrationum, in eadem ipsa re, tibi quantocyus facere copiam: ast negotiorum multitudine distentus, hoc propositum differre cogebar ad hoc usque tempus.

Adest quidem aliquod discrimen parallaxium, quas obtinuimus: sed adscribendum est id, partim diversis observationum combinationibus, partim quoque diverse assumtis locorum longitudinibus, quas minime e re fore duxi, observationum conciliandarum ergo, immutare; quippe quas, etiam uno eodemque loco captas, nimium quantum discrepare

 depre-

deprehendimus. Nec errorem in obſervando cuiquam facile imputaverim: ut enim taceam diverſitatem tuborum, oculorum, aliarumque circumſtantiarum; atmoſphæra iſta, qua Venerem cinctam eſſe jam novimus, non potuit non diverſa exhibere ingreſſus atque egreſſus phænomena, aliis hoc, aliis illud pro vero ingreſſus aut egreſſus phænomeno habentibus; id quod § VI. ulterius expoſitum invenies. Si itaque meletemata mea digna eſſe judicaveris, quæ actis veſtris inſerantur, erit id mihi gratiſſimum, imprimis, ut methodus generalis, quam hic ſiſto, et ſupputationes, quas paſſim ſparſimque exhibui, uno loco coacerventur. Molimina, quæ veſtros aſtronomos jam detineant, ad excipiendam Venerem in proximo ipſius cum Sole congreſſu, æque gratum mihi foret reſcire. Ex decreto Reg. Academiæ Scientiarum Stockholmenſis, ego Cajaneburgum iterum petam; aſt celeberrimus Upſalienſium Aſtronomus Mallet uſque ad Pello in Lapponiam contendet.

De cetero vivas diu ad vota felix; ego vero permanebo celeberrimi nominis tui

Aſſiduus cultor,

Dabam Aboæ, die 18 Decembris, anni 1767.

Andreas Planman.

Parallaxis

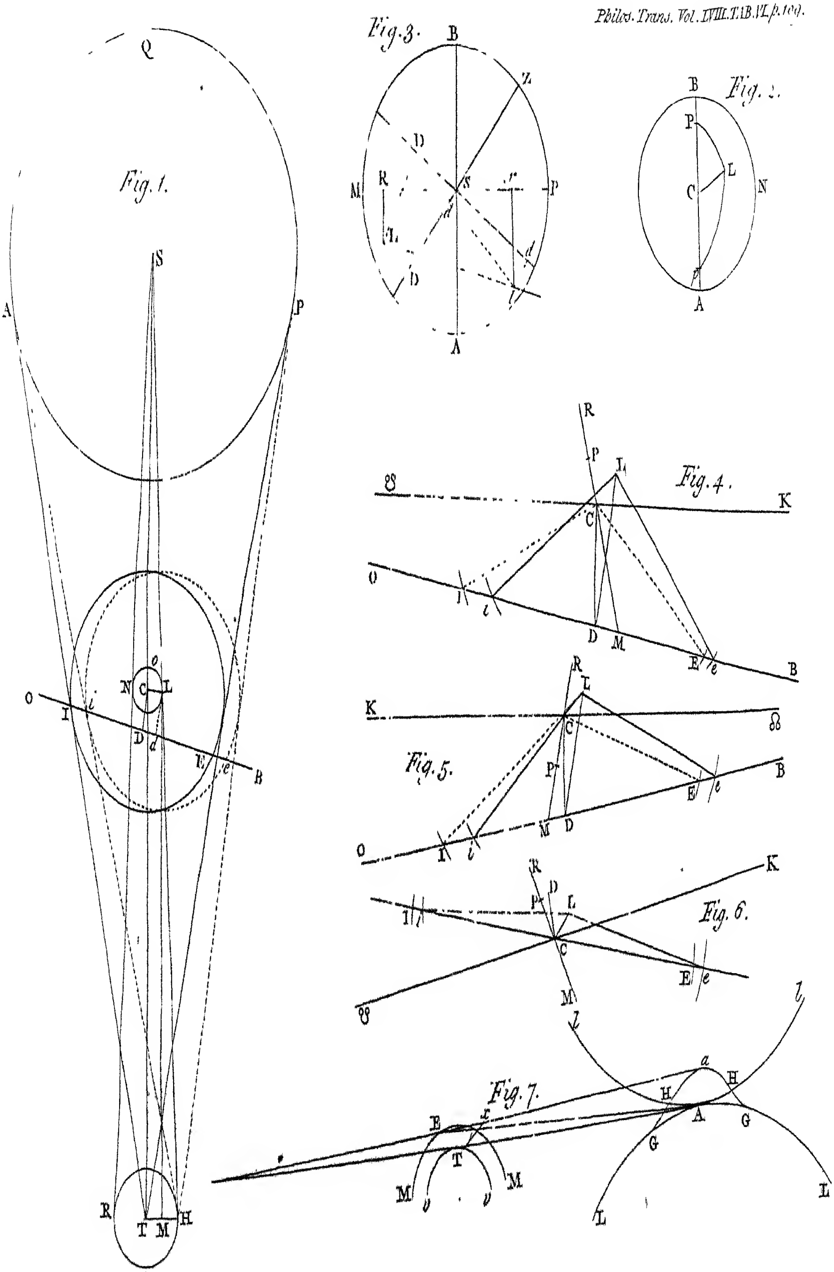


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
Fig. 5.
Fig. 6.
Fig. 7.

Parallaxis Solis ex Obſervationibus noviſſimi Tranſitus Veneris per diſcum Solarem, peculiari quadam methodo inveſtigata, ab Andrea Planman, *Phyſices Profeſſore in Academia* Aboënſi, *Academiæ Scientiarum* Stockholmenſis *Socio.*

S E C T. I.

UT methodum * expeditam ſiſterem, qua parallaxis ſolis, ex obſervationibus Veneris Solem trajicientis, inveſtigari poſſet; rem ſequenti modo concipiendam duxi. Scilicet fingo mihi e centro telluris T (TAB. VI. fig. 1.) rectas eſſe ductas ad ſingula puncta diſci ſolaris P Q A, quem, abſque notabili errore, ſub toto tranſitu fixum ſupponere licet, atque has rectas in plano ad rectam T S, centra ſolis telluriſque jungentem, normali ac tranſeunte per O B apparentem planetæ ſemitam, e centro telluris viſam, efficere diſci ſolaris projectionem I K E, cujus centrum eſt in C, ſemitam O B in punctis I et E ſecantem. Si nunc ex C recta C D ita demittatur ad ſemitam O B, ut ſit ad eclipticam normalis; videbitur, ſpectatori ex T, planeta in conjunctione cum ſole quoad eclipticam, centro ipſius in D exiſtente; aſt dimidia diſci ſui parte immerſus aut emerſus ſpectabitur planeta, dum centro ſuo ad I aut E pervenerit. Si autem ex

* Hanc methodum initio anni 1763, in Diſſertatione mea, de *Venere in Sole viſa, anno* 1761, primum evulgavi et quidem ita, ut præcipue reſpicerem hujus noviſſimi tranſitus caſum. Nunc autem illam hic generalem reddere atque ad ſingulos caſus hujuſmodi tranſituum extendere, e re omnino fore exiſtimavi.

alio

alio quocunque puncto disci terrestris, ex gr. ex H, ad finitorem lucis constituto, spectetur sol, indeque pariter agantur rectæ ad singula disci solaris puncta, mutabitur situs projectionis solis pro magnitudine et positione rectæ T H aut eidem parallelæ et proportionalis C L; adeo ut centrum solis S non jam in C, sed in L sit conspicuum; quapropter mutabuntur quoque dicta momenta, prout literæ minusculæ monstrant, existentibus I *i*, D *d*, E *e*, effectibus parallaxis, qui utique determinandi sunt, quoties observationes horum momentorum in H factæ ad centrum telluris revocentur. Ut autem hoc facili negotio conficiatur, concipio ulterius, per rectas, ex centro solis S ductas ad singula disci telluris H R Z puncta, factam esse in eodem plano, quo sol est projectus, hujus disci projectionem N *o* L, qui itidem est circulus, cujus radius C L = *parallaxi horizontali planetæ a sole*; manente T H = *parallaxi horizontali planetæ*: nam ducta L M parallela ipsi S T, erit, ob angulos T S H et M L H æquales et valde exiguos, M H = *parallaxi solis horizontali*; adeoque T M = T H — M H = C L. Si punctum H fuerit, non in finitore lucis, sed alibi in disco terræ soli obverso, erit C L aut exacte aut quam proxime æqualis *parallaxi altitudinis planetæ a sole*, prout discrimen altitudinum planetæ et centri solis fuerit aut nullum aut admodum exiguum. Effectus itaque parallaxis pendent a diverso centri disci solaris situ in circulo L O N, vel, quod eodem recidit, a diverso loco observatoris in hemisphærio H R Z; quippe hujus loci atque centri disci solis, ex hoc eodem loco conspiciendi, projectio coincidit in unum idemque punctum circuli L O N. Quapropter quæstio, de æstimandis parallaxis

effectibus, eo est reducta, ut, pro tempore quovis dato, determinetur, respectu ipsius C D, cujuscunque dati et in circulum L *o* N projecti loci telluris situs, qui locus in posterum L vocetur. In hunc finem, oportet, inveniatur C L, una cum angulo L C D, intercepto a projectura circuli verticalis et circuli latitudinis. Cum autem iste angulus pendeat ab angulo parallactico, qui a meridiano et circulo verticali comprehenditur; erit hic angulus primum inveniendus.

S E C T. II.

Exhibeat itaque circulus A N B (fig. 2.) hemisphærium telluris illuminatum et dicto modo projectum, in planum per semitam planetæ transiens (§ 1.), cujus radius C B = parallaxi horizontali planetæ a sole; exhibeat quoque A B projecturam meridiani cœlestis, in qua sit polus aut boreus P, aut australis *p*, prout conjunctio planetæ ad aut ☋ aut ☊ facta fuerit. Sit quoque C commune centrum projectionis telluris et solis e centro telluris conspiciendi; nec non L projectio loci cujuscunque dati. Atque fiat latitudo loci L = L; complementum declinationis solis C P vel C *p* = D; angulus horarius C P L vel C *p* L = A; sinus totus = 1; altitudo solis pro loco et tempore dato = C; nec non cos. A. cot. L = tang. G; eritque $\text{sin.}\,C = \dfrac{\text{sin.}\,L\ \text{cos.}\,(+D \mp G)}{\text{cos.}\,G}$ (1), in qua signa inferiora tenenda sunt, quoties fuerit D < G, alias superiora valent, excepto casu, dum A > 90°, quo signum — ipsius G abit in +, adeo ut summa ipsarum D et G sit accipienda. Statuatur

tuatur ulterius ang. parallacticus PCL vel pCL = Q, prodibitque ſin. Q $= \frac{\text{ſin. A. coſ. L}}{\text{coſ. C}}$ (II).

S E C T. III.

Quod attinet ipſam CL, erit iſta ſaltem quam proxime æqualis parallaxi altitudinis planetæ a ſole (§ 1), niſi differentia altitudinum centri ſolis et planetæ fuerit vix negligenda; quo caſu, ratio habenda eſt hujus differentiæ, quippe quæ parallaxin nominatam ab iſta CL aliquantum diſcrepantem præbet. Interim tamen, etiam in hocce caſu, parallaxin altitudinis planetæ a ſole, abſque notabili errore, in ipſa CL æſtimare licet. In hunc finem differentia altitudinum centri ſolis ſideriſque jam eſt quærenda. Exhibeant igitur AB meridianum cœleſtem; PM (fig. 3.) parallelum æquatoris; ZN, $z\,n$ circulos verticales loco et tempori reſpondentes; L l loca quæcunque data planetæ ante et poſt conjunctionem aſcenſionalem; SR, S r, differentias aſcenſionis rectæ centri ſolis S atque planetæ, quas dico a; nec non LR, $l\,r$, differentias declinationis, quæ vocentur d. Dicatur ulterius angulus, quem recta, jungens centra ſolis et planetæ, facit cum parallelo æquatoris, nempe ang. LSR vel lSr, F; eritque tang. F $= \frac{d}{a}$; nec non diſtantia centrorum ſolis ſideriſque SL vel S$l = \frac{d}{\text{ſin. F}}$. Si nunc ex L, l, agantur normales LD, $l\,d$ in circulos verticales ZN, $z\,n$; erit SD vel Sd differentia altitudinum quæſita, quæ dicatur E; huic autem determinandæ inſerviet formula.

formula $E = \frac{d.\ \text{sin.}\ (T \pm Q)}{\text{sin.}\ F}$ (III.), in qua dabitur T per d et a, quæ ex obſervationibus facile eliciuntur, et Q per æquationem (II). Circa ſigna autem ſequentes regulæ probe ſunt tenendæ: nempe *ſignum + valet, ſi obſervatio facta ſit ante meridiem et ante conjunctionem, vel poſt meridiem et poſt conjunctionem in aſcenſione recta.* Aſt *ſignum — eſt adhibendum in obſervationibus antemeridianis poſt conjunctionem, et poſt meridianis ante conjunctionem eandem.* Hæc ſigna ſunt invertenda, adeo ut ſignum — in prioris, et + in poſterioris regulæ caſu valeat, quoties ſemita planetæ fuerit borealior centro ſolis; uti fit in proximo Veneris tranſitu. Dabitur itaque jam, pro loco et tempore dato, per æquationem (I) et (III) altitudo planetæ, quippe quæ $= C \mp E$: ubi ſignum — in prioris, ex + in poſterioris regulæ caſu adhibendum eſt; exceptis illinc locis diſci telluris auſtralioribus centro ſolis, et hinc eodem centro borealioribus locis, dum dant F + compl. $Q \geq 90°$; atque hæc exceptio probe erit obſervanda, quoties ſemita planetæ, ad alterutrum nodum, fuerit auſtralior centro ſolis. Si vero planeta latitudine boreali trajiciat ſolem, exceptio locorum reſpectu eſt invertenda, adeo ut ſignum + in caſu prioris regulæ valeat, quoties locus diſci telluris, ſolis centro borealior exhibuerit F + compl. $Q \geq 90°$; ſi vero idem fiat, in caſu poſterioris regulæ, reſpectu puncti terreſtris ſolis centro auſtralioris, ſignum — tenendum eſt. Fiat jam parallaxis horizontalis planetæ a ſole $= H$, quæ in parallaxis inveſtigatione pro lubitu eſt aſſumenda, ſed ita tamen, ne a vera multum abludat; fiat quoque parallaxis altitudinis planetæ a ſole $CL = P$;

 atque

atque erit $P = H. \text{cof.} (C \mp E)$; vel, fi E negligi queat, $P = H. \text{cof.} C$.

S E C T. IV.

Hifce præftructis, formulæ jam exhibendæ funt, ad quas effectus parallaxis circa contactuum momenta fupputentur. Sit igitur ecliptica K ☋ (fig. 4.) ad nodum defcendentem, vel K ☊ (fig. 5.) ad nodum afcendentem; N E femita planetæ apparens e centro telluris vifa; C commune centrum projecturæ telluris et folis (§ I. et II.); M R meridianus cœleftis; C D latitudo planetæ momento conjunctionis quoad eclipticam; L locus quicunque datus in difco telluris foli obverfo; atque patebit ex fupra allatis, quod fit in fig. 4. ang. $PCL = Q$; et in fig. 5. ang. $pCL = Q$; nec non recta $CL = P$. Jungantur nunc puncta D et L recta D L; et fiat $CD = n$; angulus, quem meridianus facit cum ecliptica, nempe in fig. 4. R C ☋ vel in fig. 5. R C ☊ $= b$; nec non ang. $LCD = r$; prodibitque duplicis formæ ipfius r valor, prout femita planetæ ad hunc vel illum nodum, fuerit folis centro vel auftralior vel borealior: nempe fi femita fuerit ad ☋ auftralior, vel ad ☊ borealior centro folis, erit $r = 90° + b - Q$, in qua loco ipfius b fumendum eft complementum ejus ad 180°, quoties obfervatio fuerit antemeridiana. Aft exiftente femita planetæ ad ☋ borealiore, vel ad ☊ auftraliore folis centro, erit $r = Q \mp b \pm 90°$, in qua figna fuperiora in poftmeridianis, et inferiora in antemeridianis obfervationibus adhibenda funt. Statuatur ulterius $\frac{180° - r}{2} = t$; nec non $\left(\frac{n - P}{n + P}\right)$ tang. $t =$ tang. x; atque fiat ang. $CDL = y$; eritque $y = t \pm x$, in qua fignum $-$ obtinet locum, quoties

$n > P$;

$n > P$; $DL = \frac{P\,\text{ſin.}\,r}{\text{ſin.}\,y}$. Cumque datur angulus ſemitæ planetæ cum circulo latitudinis, qui dicatur e, adeo ut ſit in fig. 4. ang. $EDI = e$, et in fig. 5. ang. $CDE = e$; dabitur quoque hinc ang. LDF vel ang. LDE. Si jam centro L et radio, æquali ſummæ vel differentiæ ſemidiametrorum ſolis et planetæ, qui dicatur m, fiant ſectiones, *i*. *e*. in ſemita; erit, ob motum planetæ retrogradum, punctum orientalius i locus centri planetæ dum ſpectari in L immergere incipit vel deſinit; punctum vero occidentalius e, pro loco centri planetæ, circa contactus emerſionis, habendum eſt. Ut autem hi contactus calculo exhibeantur, determinandum erit latus Di vel De in triangulo jam dato DLi vel DLe. Fiat igitur $e \mp y = u$, in qua circa ſigna tenendum, quod exiſtente ſemita ſideris ad ☋ auſtraliore, vel ad ☊ borealiore ſolis centro; ſignum — adhibendum erit in obſervationibus poſtmeridianis, excepto caſu, quo $Q > 90° + b$; ſignum vero + valet in obſervationibus antemeridianis, niſi fuerit $Q < 90° - b$. Quoties autem ſemita planetæ ad hos nodos tenuerit ſitum oppoſitum, ordine inverſo adhibenda ſunt hæc ſigna: nempe + in poſtmeridianis, et — in antemeridianis obſervationibus; niſi dederint iſtæ $Q > 90° + b$ et hæ $Q < 90 - b$. Poſita nunc $\frac{P.\,\text{ſin.}\,r.\,\text{ſin.}\,u}{m.\,\text{ſin.}\,y} = \text{ſin.}\,z$; prodibit $Di = \frac{m}{\text{ſin.}\,u}\,\text{ſin.}\,(u \pm z)$, (A); nec non $De = \frac{m}{\text{ſin.}\,u'}.\,\text{ſin.}\,(u' \mp z')$, (B); quarum (A) immerſionis, (B) autem emerſionis contactuum ſupputationibus inſervit *.

* Ad hanc methodum, in diſſertatione mea ſupra citata (§ 1.), omnium primo exigebam calculum magni Halley, qui primus

COROL. I.

Si centro C et radio = *m*, fiant ſectiones in ſemita I et E; et ſi ponatur P = *o*; coincident puncta L, *i* et *e* cum C, I et E reſpective; atque habebitur

nos docuit, parallaxin ſolis exactiſſime determinatum iri, per obſervationes tranſitus ♀ ſub diſco ☉ (vide Tranſact. Vol. XXIX. p. 454, &c.); ut ſic mihi conſtaret, quo jure vir hiſce, de variis ſcientiis maxime promeritus, erroris, in deſignatione locorum commiſſi, ab aliis atque aliis accuſaretur. Calculo itaque ſubducto ad elementa, a celeberr. Halley adhibita, obtinui, per formulas (A) et (B), pro loco latitudinis borealis 22°, ſolemque ſub medio ♀ tranſitu ſibi verticalem habente, $Di + De = 1716''$, quæ, in tempus converſa, præbet moram apparentem ♀ intra ſolem $7^h\ 9'$. Aptato autem calculo ad meridianum oppoſitum et latitudinem borealem 56°, inveni $Di + De = 1775'',7$; unde mora hic $7^h\ 23'\ 56''$, quæ cum iſta collata, præbet differentiam moræ = $14'\ 56''$, paucis duntaxat ſecundis differentem a calculo Halley, exhibente moram ad Nelſoni portum $15'\ 10''$ majorem iſta ad oſtia Gangis. Aſt correctis elementis calculi per recentiores tabulas aſtronom. obtinui, reſpectu prioris loci, $Di + De = 1316'',7$, adeoque moram ibi $5^h\ 40'\ 10''$ in poſteriori autem loco mora obtineri non potuit, ob Venerem e ſole ibi oriente jam egreſſam. Aſſumto itaque in eodem meridiano loco paulo boreali ore, latitudinis nempe 60°, prodiit $Di + De = 1370'',5$, unde mora ibi $5^h\ 42'\ 38''$, duobus ſolummodo minutis cum dimidio circiter excedens priorem illam moram; omnino ut celeber. De L' Iſle primus deprehendit. Proinde celeber. Halley rite argumentatus eſt ex elementis, quæ adhibuit; nec error ei, ſed elementis adſcribendus eſt, imprimis vero latitudini ♀ in ſole, quæ poſtea magis quam duplo major deprehenſa eſt: pro cujus diverſitate, diverſam quoque fore differentiam moræ, Halley ipſe haud obſcure loc. cit. indicavit. Subductionis vero angulorum 8° 28′ et 6° 10′, alterius ab altero factæ, mentionem facere haud meretur; cum inde vix aliquot ſecundorum error in calculum emanaverit.

D I

$DI = \frac{m}{\text{ſin.}\ e}$ ſin. $(e \pm c)$, (C); atque $DE = \frac{m}{\text{ſin.}\ e}$ ſin. $(e \mp c)$, (D), exiſtente ſin. $c = \frac{n.\ \text{ſin.}\ e}{m}$. Ad harum formularum tenorem contactus e centro telluris ſpectati ſupputentur. Signa vero æquationum (A), (B), (C) et (D) ita obſerventur, ut ſuperiora valeant, ſi planeta, ad ☋ auſtrali, vel ad ☊ boreali latitudine ſolem trajiciat. Ad latitudinem autem planetæ in ſole, in his nodis oppoſitam, ſigna inferiora ſunt tenenda.

COROL. II.

Effectus itaque parallaxis evadet circa immerſionem $= \frac{m}{\text{ſin.}\ u'}$. ſin. $(e \pm z) - \frac{m}{\text{ſin.}\ e}$. ſin. $(e \pm c)$, qui in tempus converſus, auferendus, ſi negativus: ſi vero poſitivus fuerit, addendus erit momento obſervationis, quo habeatur momentum illud ad centrum telluris reductum. Circa emerſionem autem erit parallaxis effectus $= \frac{m}{\text{ſin.}\ u'}$. ſin. $(u' \mp z') - \frac{m\ \text{ſin.}\ (e \mp c)}{\text{ſin.}\ e}$, qui in tempus mutatus, ſi negativus, addi momento obſervationis: ſi verò poſitivus evadat, eidem demi debet, ut habeatur momentum obſervatum ad centrum telluris reductum.

COROL. III.

Si $n = o$, i. e. ſi ſemita planetæ centrum ſolis trajiceret; coincidente tunc puncto D cum C (fig. 6.), obtinebitur $C\,i = \frac{m}{\text{ſin.}\ s}$ ſin. $(v \pm s)$ pro immerſione,

atque

atque C $e = \frac{m}{\text{ſin.}\, v'}$ ſin. $(v' \mp s')$ pro emerſione. In æquationibus autem (C) et (D) (Cor. I.), evaneſcit nunc c; quare pro centro telluris relinquitur $CI = CE = m$. Atque hinc erit effectus parallaxis, pro hoc caſu, circa immerſionem $\frac{m}{\text{ſin.}\, v}$ ſin. $(v \pm s) - m$; nec non circa emerſionem $\frac{m}{\text{ſin.}\, v'}$. ſin. $(v' \mp s') - m$. Quod ſigna attinet, ſuperiora circa tam ☋ quam ☊ tenenda ſunt, quoties obſervatio antemeridiana $Q \succ 270° - b - e$, aut poſtmeridiana $Q \succ 270° + b$ non dederit; in his enim caſibus ſigna inferiora valent. Præterea monendum eſt, me poſuiſſe ſin. $s = \frac{P\ \text{ſin.}\, v}{m}$; atque $v = e \pm r$, ubi $+$ in obſervationibus antemeridianis, et $-$ in poſtmeridianis obtinebit locum, niſi iſtæ $Q \prec 90° - b$, et hæ $Q \succ 90° + b$ dederint. Ceterum pro v, exceſſus ipſius ſupra 180° ſumendus eſt, quoties caſus $Q \succ 270° - b - e$, aut $Q \succ 90° + b$ occurrerit.

SECT. V.

Expoſita ſic et ad ſingulos caſus extenſa methodo, quæ in hujuſmodi diſquiſitionibus commode adhibeatur; obſervationes jam ſunt adferendæ, quibus in parallaxi ſolis inveſtiganda uſus ſum. Ecce igitur in hunc finem ſequentem tabellam, in qua per *contact.* 1. immerſionem totalem; per *contact.* 2. emerſionis initium; et per *contact.* 3. emerſionem totalem, deſignatum volui. Quod longitudines locorum, ad meridianum Pariſienſe relatas, quæ comparent in ſecunda columna, attinet; pleraſque iſtarum tales adhibui, quales ab aſtronomis jam pridem ſunt ſtabilitæ: excepta

cepta logitudine *Bononiensi*, quam clariss. Canterzanus in epistola ad Hieronymum Saladinum, anno 1764 data, ex disquisitione celeberrimi Zanotti, non majorem 35′ 53″ esse evincit. In longitudines autem *Capitis B. Spei*, *Tobolii* et *Selenginski* inquisivit celeberr. Wargentin in *Actis Stockh.* pro anno 1763, unde istas desumsi. Denique quod attinet longitudinem *Pekini*, istam 7^h 35′ 50″ non excedere, celeber. Rumousky in tractatu, quem *investigationem parallaxeos solis* vocat, evincere conatus est.

Nomina

Nomina *locor.* et Obfervatorum		Longitudo.	Contact. 1	Contact. 2.	Contact. 3.
		h ′ ″	h ′ ″	h ′ ″	h ′ ″
Cap. B. Spei.	Mafon	1 4 25 or.	———	9 39 52	9 57 23
	Dixon	———	———	9 39 48	9 57 21
Bononiæ.	Frifius	0 35 53	———	9 4 56	9 22 59
	Marinus	———	———	9 4 58	9 23 0
	Matheucius	———	———	9 4 58	9 23 7
	Com. Caffalio	———	———	9 5 0	———
Parifiis.	Le Monnier	0 0 0	———	8 28 19	8 46 47
	De la Lande	———	———	8 28 26	8 46 50
	Clouet	———	———	8 28 27	8 46 55
	Baudouin	———	———	8 28 27	8 46 46
	Fouchy	———	———	8 28 29	8 46 40
	Ferner	———	———	8 28 29	8 46 40
	Meffier	———	———	8 28 29	8 46 37
	De la Caille	———	———	8 28 37	8 46 49
	Merville	———	———	8 28 40	8 47 4
	Condamine	———	———	8 28 42	8 46 49
	Maraldi	———	———	8 28 42	8 46 54
Gottingæ.	Mayer	0 30 11	———	8 58 26	9 16 54
Grenovici.	Blifs	0 9 10 oc.	———	8 19 0	8 37 9
	Short	———	———	8 18 50	8 37 28
	Dollond	———	———	8 18 58	8 37 14
	Canton	———	———	8 18 58	8 37 21
Calmariæ,	Wykftrom	0 56 13 or.	3 33 1	9 23 40	———
Stockholmiæ.	Wargentin	1 2 50	3 39 23	9 30 8	9 48 9
	Klingenftierna	———	3 39 29	9 30 11	9 48 8
Upfaliæ.	Bergman.	1 1 10	3 37 43	9 28 9	9 46 30
	Mallet	———	3 37 56	9 28 2	9 46 29
	Melander	———	3 38 2	———	9 46 29
	Stromer	———	3 38 5	9 28 0	———
Cajaneburgi.	Planman	1 41 30	4 18 5	10 7 59	10 26 22
Torneæ.	Hellant	1 27 39	4 3 59	9 54 8	10 12 22
Tobolii.	Chappe	4 23 45	7 0 30	12 49 23	13 7 42
Selengifki.	Rumoufky	6 57 5	———	15 21 36	15 39 42
Pekini.	Dollier	7 35 50	10 10 27	15 59 59	16 17 57

SECT.

SECT. VI.

Discimus ex tabella præcedenti ingens extitisse discrimen observationum, ab exercitatissimis astronomis, uno eodemque loco captarum. Etenim Maraldinum momentum contactus interioris a Monnierio 23 secundis differt. Contactus vero exterior a clariss. Messier captus totis 27 secundis, antevertit istum Mervilleanum. Vix minor deprehenditur dissensus observationum contactus immersionis: nam hujus contactus momenta a celeberr. Stromer et Bergman Upsaliæ capta 22 secundis discrepant. Hinc dissensus observationum moræ Veneris intra solem dodrantem minuti primi excedere potest; id quod mora Stromeriana cum Wargentina collata satis ostendit. Tanti autem discriminis causam eo minus petendam esse, ex diversa tuborum longitudine, existimaverim, quo certius constat tubos, a celeberrimis his viris adhibitos, longitudine parum admodum discrepasse; nam differentia longitudinis tubi Maraldini et Monnierii erat solummodo trium pedum. Messier atque Merville usi sunt telescopiis 60 atque 72 pollicum. Quid? quod celeb. Wargentini atque Bergmanni tubus unius duntaxat pedis longitudine excederet tubum Stromerianum. Itaque non ex diversa tuborum longitudine, sed ex alio fonte, nempe ex radiorum refractione, in atmosphæra Veneris facta, imprimis derivanda est enormis ista observationum discrepantia. Ut autem hoc clarius constet, exhibeat arcus L A L (fig. 7.) circa interiorem, et *l* A *l*

 circa

circa exteriorem contactum, partem limbi solaris; arcus VTV particulam ♀ immergentis aut emergentis; nec non cingulum VMMV partem atmosphæræ ♀. Ponatur nunc ♀ eum obtinuisse situm, ut recta, jungens punctum limbi solis A et oculum spectatoris in O, tangat discum ♀ in T; quare radii, qui ex A ad O emittuntur, in atmosphæra ♀, quam trajiciunt, bis refringuntur, nempe in R et E; quamobrem observatori in O punctum A videbitur in *a*. Cumque duplex hæc refractio competit reliquis quoque punctis huicce A adjacentibus, perspicuum est, circa contactum interiorem exhiberi in limbo solis gibbum quendam luminosum GAG, ast, circa exteriorem, discum solarem deficere figura quadam H*a*H. Gibbus autem iste erit, circa immersionem, maximus eo ipso momento, quo recta RE, radii refracti via in atmosphæra ♀, tangit ♀ in T; inde vero decrescit, usque dum recta AO atmosphæram ♀ tangit, quo momento gibbus iste evanescit, limbi solaris circularis figura restituitur, ac Venus, aliquantum jam intra solis discum demersa, genuina solis luce circumdatur. E contrario gibbus hic, circa emersionem, crescit ab eo momento, quo radius, ex A ad O delatus, atmosphæræ ♀ primum occurrit, usque ad id momentum, quo RE tangit Venerem lumenque gibbi disparet. Hinc mihi valde verosimile videtur, alios posterius illud momentum, quo gibbus disparuit, contactui interiori assignasse; alios iterum id momentum, quo generabatur gibbus, pro immersione totali, vel emersionis initio habuisse; inprimis quia gibbus, ♀ immergente, tum maximus erat, et ea propter cum lumine limbi solaris facile confundendus ab illis, qui ad gibbosam istius figuram non animadverterint. Venere autem emer-

3 gente,

gente, gibbus inter generandum minimus erat, quapropter, ceu obſcurior reliquo limbo ſolis, ſpecie emergentis Veneris obſervatorem fallere potuit. Hujus refractionis phænomena obſervatores non potuit non æque ſuſpenſos tenere de rite capiendo momento egreſſus totalis: nam eo ipſo momento, quo crederes ♀ a ſole divulſum iri, ſpectanda relinquitur, in margine diſci ſolaris, figura anguli cujuſdam obtuſi H *a* H, qui magis magiſque factus acutus, ictu oculi evanuit. Hoc phænomenon Stockholmiæ binis, et Upſaliæ ſingulis obſervatoribus omnino erat momentaneum; et veroſimile mihi quidem occurrit, plures alios idem momentum pro totali egreſſu habuiſſe: unde factum eſt, quod his ♀ diutius in ☉ videbatur, quam aliis, qui ad figuram hanc, margini ſolari ſuperimpoſitam, non adimadverterunt. Quemadmodum itaque hinc jam pateſcere exiſtimaverim, palmarium obſervationum diſcrimen, variis iſtis phænomenis, a refractione radiorum in atmoſphæra ♀ pendentibus, adſcribendum eſſe, ſi excipias diſſenſum, qui in hiſce debetur diverſitati tuborum aliarumque circumſtantiarum; ita quoque ſperaverim, hæc unumquemque cautum reddere, in capiendis ingreſſus et egreſſus momentis, in proximo ♀ tranſitu. Meo quidem judicio, pro ingreſſu totali, capiendum erit momentum, quo filum luminis ſolaris coaleſcit, peripheria ejus circulari reſtituta. Aſt, pro egreſſus initio, habendum eſt momentum totalis diſparitionis luminis, ad marginem ♀ occidentalem factæ. Egreſſus vero totalis momentum habeatur plenaria limbi ſolaris reſtitutio, quo nempe phænomenon illud nuper deſcriptum et a refractione pendens evaneſcit. Vellem inſuper, ut id momentum quoque conſignare-

R 2 tur,

tur, quo circa contactum exteriorem figura ista angularis in margine solari formatur, et quo circa interiores contactus ibidem gibbus generatur, una cum fascia quadam nigricante, quam nonnulli, ultima vice, ex ♀ limbo, in directionem gibbi, simul exeuntem deprehenderunt. Si vero omnia exacte æstimare non daretur; ad minimum ista phænomena sunt accuratissime consignanda, quæ in capiendis ingressus egressusque momentis conspiciuntur. Alias enim selectionem observationum, si rite instituatur, frustra tentaveris; cum tamen ejusmodi selectio maxime e re foret, in disquisitione parallaxis solis, quæ similiter captis observationibus erit peragenda.

S E C T. VII.

Sed exhibeantur jam parallaxes solis horizontales, quas mihi dabat comparatio observationum utriusque contactus, ad *Caput B. Spei*, et singulorum trium contactuum *Pekini* captarum, cum respondentibus. Valores autem quantitatum prout mihi aut ex observationibus, aut ex *Tab. Astron.* constabant, adhibui sequentes: nempe D $= 67^\circ\ 18'\ 26''$; H $= 20'',6$, supposita parallaxi solis horizontali $8'',2$, qua usus sum; $n = 579'',6$; $b = 83^\circ\ 51'$; $e = 81^\circ\ 30'$; diametrum solis $= 31'\ 35''\ \frac{1}{2}$; Veneris vero $= 57\frac{1}{2}$; motumque horarum ♀ in semita apparenti $= 4'\ 1''$.

Nomina

Nomina Obſerv.	Parallaxis Solis Horizontalis ſupputata ex Obſervationibus peractis.				
	Ad Caput B. Spei.		Et Pekini.		
	Cont. 2.	Cont 3.	Cont. 1.	Cont. 2	Cont. 3.
	″	′	″	″	″
Friſius.	8 ,18	8 ,20	—	8 ,86	8 ,57
Marinus.	8 ,13	8 ,18	—	8 ,92	8 ,59
Matheucius.	8 ,13	8 ,01	—	8 ,92	8 ,80
Comes Caſſalio.	8 ,08	—	—	8 ,98	—
Le Monnier.	8 ,62	8 ,21	—	8 ,20	8 ,60
De La Lande.	8 ,53	8 ,16	—	8 ,36	8 ,65
Clouet.	8 ,51	8 ,09	—	8 ,39	8 ,73
Baudouin.	8 ,51	8 ,23	—	8 ,39	8 ,58
Fouchy.	8 ,49	8 ,29	—	8 ,43	8 ,42
Ferner.	8 ,49	8 ,29	—	8 ,43	8 ,42
Meſſier.	8 ,49	8 ,20	—	8 ,43	8 ,32
De La Caille.	8 ,35	8 ,18	—	8 ,62	8 ,63
Merville.	8 ,30	7 ,93	—	8 ,68	8 ,95
Condamine.	8 ,28	8 ,18	—	8 ,77	8 ,63
Maraldi.	8 ,28	8 ,11	—	8 ,77	8 ,71
Mayer.	8 ,24	7 ,80	—	8 ,88	9 ,22
Bliſs.	8 ,46	8 ,43	—	8 ,48	8 ,26
Short.	8 ,58	8 ,07	—	8 ,24	8 ,82
Dollond.	8 ,49	8 ,33	—	8 ,45	8 ,39
Canton.	8 ,49	8 ,22	—	8 ,45	8 ,59
Wykſtrom.	8 ,39	—	8 ,52	8 ,63	—
Wargentin.	8 ,20	8 ,29	8 ,06	9 ,20	8 ,51
Klingenſtiern.	8 ,15	8 ,31	8 ,48	9 ,35	8 ,48
Bergman.	8 ,49	8 ,24	8 ,06	8 ,69	8 ,64
Mallet.	8 ,58	8 ,26	8 ,96	8 ,10	8 ,61
Melander.	—	8 ,26	9 ,36	—	8 ,61
Stromer.	8 ,62	—	9 ,56	8 ,00	—
Planman.	8 ,38	8 ,10	8 ,64	8 ,83	9 ,20
Hellant.	8 ,28	8 ,17	8 ,28	9 ,26	9 ,07
Chappe.	8 ,43	8 ,12	8 ,39	—	—
Rumouſki.	8 ,26	8 ,12	—	—	—
Dollier.	8 ,46	8 ,37	—	—	—
Per Medium.	8 ,38	8 ,18	8 ,63	8 ,62	8 ,65.

Sumto

Sumto nunc medio horum mediorum, evadit ſolis parallaxis 8″,49. Si autem rejiciantur parallaxes, quæ prodeunt ex Pekinenſium obſervationum comparatione, ob longitudinem *Pekini* nondum certo ſtabilitam; relinquitur ſolis parallaxis 8″,28, ceu medium, ex obſervationibus ad *Caput B. Spei* factis, deductum.

SECT. VIII.

Ut autem conſtaret, quo jure celeberr. Pingré in tractatu, quem *Parallaxe de Soleil* vocat, dubias reddere conatus eſt obſervationes ad *Caput B. Spei* peractas, inſtituendas eſſe duxi plures comparationes, quarum tamen iſtæ, obſervationibus moræ ♀ intra ſolem ſuperſtructæ, ob effectuum parallaxis exiguam, obſervationum vero nimiam diſcrepantiam, hic adferri non merentur. Itaque eæ comparationes exhibendæ reſtant, quæ nituntur mediis utriuſque contactus obſervationibus et *Pariſiis* et *Bononiæ* captis, in quem finem, ecce ſequentem tabellam:

Nomina Obſerv.	Pariſiis. Cont. 2.	Pariſiis. Cont. 3.	Bononiæ. Cont. 2.	Bononiæ. Cont. 3.
	″	″	″	″
Rumouſky.	8,00	7,98	8,44	8,11
Chappe.	8,83	7,88	9,02	8,12
Hellant.	7,81	8,00	8,63	8,25
Planman.	8,20	7,75	9,00	8,08
Strömer.	9,52		10,25	
Mallet.	9,33	8,37	10,10	8,66
Melander.		8,37		8,66
Bergman.	8,67	8,29	9,65	8,59
Wargentin.	7,13	8,69	8,43	8,75
Klingenſtierna.	6,83	8,78	8,20	8,83
Per Medium.	8,22	8,23	9,08	8,45

Hinc

Hinc iterum per medium habetur ſolis parallaxis 8″,49. Rejecta autem columna tertia, ceu maxime diſcrepante, dabunt reliquæ ſolis parallaxim 8′,30, quæ cum parum admodum abludat ab iſta 8′,28, quam maximi momenti obſervationes præbebant (§ VII.); vi noviſſimi tranſitus Veneris, parallaxis ſolis horizontalis quam proxime ſtatuenda eſt 8″,28, ſalvis differentiis meridianorum, quas adhibui. De cætero, ſunt mihi rationes, quæ parallaxin potius minuendam, quam augendam eſſe ſuadent: ſed mitto has, donec proximus Veneris tranſitus ſub diſco ſolis, modo ex voto ſuccedant obſervationes, in rem ſubtiliſſimam exactius inquirendi anſam nobis ſubminiſtraverit.

XVII. *A short Account of the Manner of inoculating the Small Pox, on the Coast of* Barbary, *and at* Bengal, *in the* East Indies, *extracted from a Memoir written in* Dutch, *by the Reverend Mr.* Chais, *at the* Hague: *by* M. Maty, *M. D. S. R. S.*

Read April 14, 1768.

HAVING long thought that the Arabs, who, about the middle of the sixth century, were the first who wrote upon the small-pox, were likewise the first inventors of the method to prevent the fatal consequences of that cruel disorder, I was very desirous to get what informations I could concerning the introduction of inoculation in Africa, and in the East Indies.

About twenty years ago, Cassen Aga, a Tripolitan ambassador at London, informed the people about him, that inoculation was universally practised, as well at his court, as at Tunis and Algiers; but that no certain account could be given, either of the introducers of the method, or of the place from whence it took its rise.

One of the chief ministers of state in Holland was so good, on this information, and at my desire, to send a few queries on that subject, drawn up by myself, to a gentleman, who, for several years, has resided with a public character at Algiers. The following is a summary of his answers to my queries.

" The small-pox is, as well as in Holland, a contagious distemper at Algiers, Tunis and Tripoli,

" and

"and fully as destructive. In order to avoid the bad consequences of the natural disorder, many people have recourse to inoculation, which there is performed in a very different manner from what is used in our country. The person, who intends to be inoculated, having found out a house, where the small-pox is, and is of a good sort; goes to the bed of the sick person, if he is old enough, or, if a child, to one of his relations; and speaks to him in the following manner; *I am come here to buy the small-pox:* the answer is, *buy if you please.* A sum of money is accordingly given, and one, three, or five pustules (for the number must always be an odd one, not exceeding five), extracted whole, and full of matter. These are immediately rubbed upon the skin of the hand, between the thumb and fore-finger. This is sufficient to communicate the infection; and as soon as it begins to take effect, the inoculated patient is put to bed, carefully covered with red blankets; and heating medicines are given him with some honey of roses. He is allowed goat's broth for his nourishment, and for his drink an infusion of some herbs; notwithstanding this treatment, it seldom happens that the small pox procured in this manner has any bad consequences; and almost never that any body dies of it; but hitherto the proportion of the mortality in the natural, to that in the artificial way, has not been ascertained. Lastly, though the time when this practice was introduced in Africa be unknown, yet it is there very old; and the Arabs, are generally thought to have been the inventors of it."

From this account it plainly appears; 1. that in Africa the operation is performed as it is in Wales, by the rubbing in of the matter, and that this is done to prevent the fatal consequences too often following the natural infection; 2. that this inoculation is generally successful, notwithstanding the heat of the climate, and the bad management of the patients; and 3. that the origin of it is very ancient, and ascribed to the Arabs.

Before I had received these informations from Algiers, I had engaged some friends settled in three different parts of the East Indies, to procure me some accounts from thence, upon the same subject. I, at last, received an answer from one of them, who resides at Patna, in the province of Behaar, 180 leagues from Bengal.

"I have sent for several physicians, to be informed "of the things you seem desirous to know about ino-"culation; the practice is hitherto not used in this "province; but having met with a Bengalian "doctor, he gave me the following account.

"Though the first introduction of the operation at "Bengal is now unknown, it has been in use in that "country for a very long time, and is performed in "two different ways.

"For the first, some of the variolous matter of "a good kind having been gathered is kept for use. "When a child is to be inoculated, the skin between "some of the fingers is pricked by means of two "small needles joined to one another. After having "rubbed in a little of the matter upon the spot, a cir-"cle is made by means of several punctures, of the "bigness of a common pustule; and matter is again "rubbed over it. The wound is then dressed with

"lint; a fever ensues, and after some days, the "eruption, which if the fever has been strong is ob- "served not to be very copious. To excite the fe- "ver, the patient is made to bathe in a tub of "water.

"As this way of managing the operation is very "painful, a more easy one has been invented for peo- "ple of quality and substance. A little of the matter "is mixed with sugar, and swallowed by the child in "any sweet and pleasant liquid. The same effect "is produced, but the first method is thought to be "the best."

The writer of this letter ought certainly to have been more particular in his inquiries; he might have asked whether any preparation previous to inocula- tion is used, and of what kind; what treatment the patients undergo after the operation; and lastly, how far the event warrants the goodness of the method. It appears however, from what he says, that the peo- ple of Bengal have for a long while had recourse to inoculation, in order to avoid the dreadful consequen- ces of the natural distemper in their country; and it is to be wished that farther inquiries be made, both there and elsewhere, about a subject which so nearly concerns the good of mankind.

XVIII. CROTON Spicatum, *nova Plantæ Species ex* America, *quam Descriptione ex icone illustravit* Petrus Jonas Bergius, *M. D. Hist. Nat. et Pharm. Profess.* Stockh. *R. Colleg. Med. Assessor, Reg. Acad. Scient.* Stock. *Membr.*

Read April, 21, 1768.

SCIENTIÆ rei herbariæ ulteriori perfectioni nihil perinde conducere arbitror, atque idoneas plantarum novarum minusve cognitarum descriptiones, præsertim ubi bonæ etiam accesserint icones. Ut enim in omnibus aliis scientiis magnopere expedit, quarumcunque rerum eo pertinentium ignorantiam vel incertitudinem, quoad fieri potuerit, tolli, ita valde quoque opportunum scientiæ contingit botanicæ, quotiescunque novæ rarissimæque plantæ in lucem proferuntur, et quidem ita proferuntur, ut quælibet partes earum essentiales, quoad licuerit, et illucide adumbrentur. Certe quidem hoc generatim de omnibus valet plantis, ad quodcunque demum stirpium pertinuerint genus; nulla enim unquam detecta fuit species, quin attentione fuerit dignissima; tamen solent speciatim illæ ipsæ noviter detectæ species impensiori excipi eruditorum attentione et applausu, quæ ad genus quoddam pertinent, in quo una pluresve species virtute usuque aliquo singulari, vel medico vel œconomico, dudum inclaruerunt, quippe quoniam suspicari fas sit, in cunctis speciebus affinibus quodammodo consonas, saltem non multum alienas, inesse vires.

 Hocce

Hocce respectu operæ profecto pretium fuerit, rarissimam quandam sistere speciem, ad amplum illud pertinentem stirpium genus, quod utraque potissimum India profert, quodque botanicis *Croton* audit. Etenim non una duntaxat, sed plures species, sub unius generis vexillo comprehensæ, ob eximium in medicina aut œconomia usum multo dudum nomine sunt, quod quidem satis ii norunt, qui ex merito æstimare didicerunt *Croton Cascarillam*, *Croton febiferum*, *Croton Tiglium*, *Croton tinctorium*, atque *Croton aromaticum*. Hinc ubi nova eidem huic generi accedit species, adeo non id a botanicis ferri poterit indifferenter, ut potius non possint non pronum eo advertere animum oculosque. Et vero etsi nondum quidquam de nulla novæ stirpis virtute constet, tamen id movere potuerit neminem, qui, uti par est, cogitaverit, præcedere semper ante oportere primam rei cujusque notionem, quam vel minima de qualitatibus ipsius oboriatur quæstio.

Hisce jam pensitatis, propius me ad ea, quæ de planta mea observanda habui, confero: Et quidem ideo illam novam dicebam, quod cum manca duntaxat ac admodum imperfecta mentione ad notitiam botanicorum pervenit, adeoque nec ab ullo adhuc systematicorum in album stirpium cognitarum est relata. Certe Loeflingio nostrati in America peregrinanti, illam ipsam visam fuisse, haud obscure ex itinerario ejus patet, ubi tamen non nisi breviter et quasi per transennam unam alteramque ejus tradit notam, quanquam quidem fusius sine dubio reliqua persecutus fuisset, nisi fato occubuisset præmaturo. Ceterum nec multum refragabor, si quis contenderit, arborem illam mali folio, quam profert doctiss. H. Sloane, *Hist. Jam. tom.* ii. *p.* 30. *tab.* 174. *f.* 1.

Croton

Croton esse spicatum, tametsi non negem, unam alteramque discrepantiam cerni posse evidentiorem, quod fortasse maximam partem pictori tribuendum; interim tamen suadent convenientiam imprimis spicæ terminales, ut reliqua taceam.

Icon adjecta fruticis ramulum exactissime refert magnitudine naturali, lectum ante biennium Havanæ a chirurgo N. RUDOLPH.

TAB. VII.

CROTON.

CROTON (*spicatum*) foliis ovatis glabris, ramis nudis, florum racemis spicatis terminalibus. Croton foliis ovatis, floribus spicatis, stylis multifidis depresso patentibus, frutescens. LOEFL. *It.* *p.* 234. *n.* 50.

Mali folio arbor, artemisiæ odore, flore pentapetalo spicato. SLOAN. *Hist.* *Jam.* ii. *p.* 30. *t.* 174. *f.* 1. vix bona. *Cat.* *Jam.* 139. RAJ. *dendr.* 17. Habitat Havanæ in America.

DESCR. *Caulis* fruticosus, ramosus. *Rami* subdeterminati, cinerei, subrugosi, erecti, nudi, cicatricibus obsoletis, ramulosi. *Ramuli* consimiles; tenelli, glabri, sulcati. *Folia* solummodo in ramis tenellis, alterna, ovato oblonga, integerrima, basi rotundata, apice sublanceolato, obtuso, firmiuscula, utrinque glabra, petiolo instar nervi longitudinalis discum folii subtus percurrente, nervis obliquis tenuioribus nervosa, bipollicaria vel paulo ultra, frequentia, petiolata, patentia. FLORES MASCULI racemoso-spicati, pedicellati. *Racemi* terminales, pyramidales, compactiusculi, solitarii, pedunculati. *Bracteæ* lineares, obtusæ,

CROTON spicatum.

J. Mynde sc.

ſæ, glabræ, ad baſin pedunculi racemi ſitæ. *Pedicelli* ſubpubeſcentes. CALYX. *Perianthium* decaphylli ſæpe ultra, imbricatum: ſquammis ſubæqualibus, ovatis, obtuſiuſculis, extus glabriuſculis, intus hirſutis: hirſutie albida. COROLLA nulla. STAMINA. *Filamenta* xii, ſæpe ultra, ſubulata, inferne hirſuta, erecta, longitudine calycis. *Antheræ* ſubrotundæ, compreſſæ, ſubtetragonæ, erectæ. *Receptaculum* ſtaminum hirſutum. FLORES FEMINEI ſolitarii, ad baſin pedunculi racemi maſculini intra bracteam ſiti pedunculati, erecti. CALYX. *Perianthium* pentaphyllum, infernum: foliolis lineari-lanceolatis, acutiuſculis, erectis, ſubæqualibus, minutiſſime punctatis, ſubſcabiuſculis. PISTILLUM. *Germen* ſubrotundum, compreſſum, ſubhirſutum. *Styli* tres, longitudine calycis, infima baſi cohærentes; ſinguli ad medium ſexfidi: laciniis ex uno puncto prodeuntibus, ſubulatis; unde ramoſi apparent. *Stigmata* xviii, obtuſa.

OBS. A Croto glabello LINN. caute diſtinguendum; etenim in illo rami folioſi, flores racemoſo-paniculati, axillares.

XIX. *Ob-*

Read April 28, 1768.

XIX. *Observations on the Barometer and Thermometer, and Account of the whole Rain in every Month of the Year* 1767, *taken at the Royal Hospital near* Plymouth: *By* William Farr, *M. D. Transmitted to* William Watson, *M. D. F. R. S.*

Month. 1767.	Barometer.	Therm.	Winds from what Quarter highest.	Rain.	General Account of the Weather.
January	Highest state 30 20 Lowest D° 28 67	D° 53 D° 24	E. by N. S. E. S. } 3	4 630	First part of the month clear, and frost; remainder much rain and stormy weather. January 1st, remarkable gust of wind E. by N.
February	H. 30 05 L. 29 04	H. 53 L. 48	S. S. W. S. W. } 3	7.000	Constant rains, only three fair days during the whole month. High winds on the 11th, 19th, and 26th.
March	H. 30 23 L. 29 28	H. 55 L. 44	N. W. W. by N. } 3	4.270	Sky serene and clear the greatest part of the month; towards the middle and latter end very heavy rains.
April	H. 30 41 L. 29 05	H. 55 L. 45	S. E. 3 S. S. E. 2″	2.250	Rain from the 20th to the 27th. Storm 21st S. E. the rest of the month very fine weather. Wind mostly E. and S. E. and sometimes N.
May	H. 30 39 L. 29 25	H. 59 L. 50	W. 3 N. N. N. W. } 2‴	2.860	First part of the month very fine weather; middle and end showery, with hail storms. Air generally sharp through the whole.
June	H. 30 32 L. 29 44	H. 64 L. 53	E. by S. E. by N. } 2″	.882	The 8th the first warm day this year; from thence to the 17th very fine weather, and in general through the month; though the air was often sharp. No high Winds this month.

July

1767. Month.	Barometer.		Therm.		Winds from what Quarter highest.	Rain.	General Account of Weather.
July	Highest state Lowest D°	29 98 29 11	H. L.	65 60	W. by N. } W. by S. } 3 S. E. } S. W } 2‴	5.560	Almost constant rains during the month; only five fair days.
August	H. L.	30 21 29 50	H. L.	69 61	N. N. W. } S. W. } 2‴	3.135	First part of the month very fine weather; calm throughout, except from the 14th to the 16th; towards the latter end sultry; 29th, thunder-storm.
September	H. L.	30 33 29 49	H. L.	67 60	N. W. 2‴ S. 2″	2.531	First part of the month constant rains; from the 18th to the latter end remarkably fine weather, and quite serene.
October	H. L.	30 23 29 15	H. L.	61 48	S. W. } W. } 3 S. W. } S. E. } 2‴	3.325	From the 8th to the 15th, weather clear and serene, with sharp frosty mornings; rest of the month rain, or hazy weather.
November	H. L.	30 37 29 04	H. 10th L.	60 42	S. W. 10th } S. S. W. 13th } 4 W. by S. 2‴	3.506	Rain frequent till the 21st; from the 8th to the 16th very squally weather; from the 21st to the end of the month fair.
December	H. L.	30 54 29 02	H. L.	53 28	E. by S. 3 N. E. 2‴	1.850	Beginning and latter end frosty weather, with little wind; middle, squally with showers.

Total 41.799, or 41 ¾ inches of rain nearly.

For the more particular account of the weather, during the late frost, vide the subsequent pages.

State of the Barometer, Thermometer, Wind, and Rain, from *December* 20, 1767, to *January* 21, 1768.

1767. Day. December.		Bar.	Ther. within doors.	Ther. without.	Wind.	Rain	Weather, with miscellaneous remarks.
20	9 a. m.	29 2	50		S. E. 2'''		Fair
	11 p. m.	29 27	50		S. E. 2''		D°
21	9 a. m.	29 56	50		E. b. S. 2'''		Clear
	11 p. m.	29 70	41		E. 2''		D°. N. B. Froſt ſet in this evening
22	9 a. m.	29 80	40		E. b. N. 2''		Froſt air very ſharp
	11 p. m.	29 86	39		N. E. 2'''		D°
23	9 a. m.	29 90	39		D° 2'''		D°
	11 p. m.	29 93	38		D° 2'''		D°
24	9 a. m.	30 1	35		D° 2'''		D°
	11 p. m.	30 0	35	30	D° 2''		Cloudy, with froſt
25	9 a. m.	29 93	35	30	N. N. E. 1		Clear and ſerene
	11 p. m.	29 75	33	28	D° 2		Cloudy
26	9 a. m.	29 66	33		D° 2'		Sleet at times
	11 p. m.	29 60	31	27	N. 1		Clear
27	9 a. m.	29 44	30		N. 0*		Clear and ſerene in the morning
	11 p. m.	29 41	29		N. 0		Snow ſince the afternoon
28	9 a. m.	29 44	29		N. 0		Clear; ſnow lies on the ground about 4 inches deep
	11 p. m.	29 55	30		N. 1		Clear
29	9 a. m.	29 77	31		N. 1		Clear and ſerene
	11 p. m.	29 74	33		S. E. 1		Cloudy; in the night ſhowers
30	9 a. m.	29 76	34		D° 1	.180	Clear and froſt
	11 p. m.	29 74	36		D° 1		D°
31	9 a. m.	29 80	35		D° 2		Fair and froſt; towards noon thaw
	11 p. m.	29 41	36		D° 2'''	.220	Rain with ſleet; and in the night ſnow
1768. January.							
1	9 a. m.	29 0	36		D° 2''	.350	Rain, heavy at times, through the forenoon
	11 p. m.	29 30	34		N. E. 2'''		Clear and froſt
2	9 a. m.	29 60	33		N. 2''		D°
	11 p. m.	30 0	33		N. 2		D°
3	9 a. m.	30 10	29	20	N. 2		D° { N. B. 20 was the loweſt ſtate of the therm. obſerved in theſe parts, without doors.
	11 p. m.	30 3	30	21	D° 2		D°

1768.

1768. Day. January.		Bar.	Ther. within doors.	Ther. without.	Wind.		Rain	Weather, with miscellaneous remarks.
4	9 a.m.	30 2	30	20	N.	2″		Fair and frost
	11 p.m.	29 92	30	22	D°	2′		Cloudy, with D°
5	9 a.m.	29 90	30		D°	2′		Clear and frost
	11 p.m.	29 80	30		D°	2′		Cloudy
6	9 a.m.	29 66	32	30	D°	2′		Snow
	11 p.m.	29 60	32		N. E.	2		Hail and sleet, with frost
7	9 a.m.	29 54	32		E.	2		Cloudy
	11 p.m.	29 49	34		E. b. S.	2		D°
8	9 a.m.	29 26	35		S. E.	2″		Rain constant, with thraw through the day
	11 p.m.	29 20	35		D°	2″	·700	D°
9	9 a.m.	29 10	36		D°	2″		Rain at times
	11 p.m.	29 14	36		E.	2‴		Rain constant
10	9 a.m.	29 34	37		N. W.	2″	·560	Snow, with rain at times
	11 p.m.	29 56	37		N.N.W.	2″		Cloudy
11	9 a.m.	29 60	36		D°	1		Fair and frost
	11 p.m.	29 68	36		W.b.N.	1		Thaw, with snow; now frost again
12	9 a.m.	29 66	36		S. E.	2		Rain from 11 a.m. constant through the day
	11 p.m.	29 50	39		D°	2		D°
13	9 a.m.	29 47	48		S.	2		Cloudy
	11 p.m.	29 36	48		S. W.	2‴		Rain since 2 p.m.
14	9 a.m.	29 30	48		D°	2″		Cloudy, with rain
	11 p.m.	29 16	49		D°	2	·480	Cloudy
15	9 a.m.	29 25	48		W.	2		Rain heavy till noon
	11 p.m.	29 50	47		W. b. S.	1		Fair
16	9 a.m.	29 80	46		D°	0		Fog in the morning; afterwards clear and serene
	11 p.m.	29 87	45		S. W.	1	·600	Clear
17	9 a.m.	29 87	45		E.	2		Close and cloudy, with small rain
	11 p.m.	29 78	45		S. W.	1		Cloudy
18	9 a.m.	29 46	45		W. b. S.	1		Rain heavy till the afternoon
	11 p.m.	29 65	44		N. W.	1		Cloudy
19	9 a.m.	29 67	44		D°	1		Clear and serene
	11 p.m.	29 56	44		D°	2″	·775	Rain since 4 p.m.
20	9 a.m.	29 60	44		N.N.W.	4		Fair with flying clouds. N. B. Hurricane from
	11 p.m.	29 75	44		D°	2		4 to 8 a.m.; when the Fame Man of War

was driven from her mooring in Hamoaze; together with the steer-hulk on the rocks, by Saint Nicholas Island.

N. B. The thermometer within doors is kept in an airy open stair case, and not affected by any fire in the House; that without was in a shed under the North wall.

N. B. From the 2d to the 7th of February was a frost, and since that time incessant rain to March 1.

XX. An

Received April 20, 1768.

XX. *An Account of Inoculation in* Arabia, *in a Letter from Dr.* Patrick Ruſſell, *Phyſician, at* Aleppo, *to* Alexander Ruſſell, *M. D. F. R. S. preceded by a Letter from Dr.* Al. Ruſſell, *to the Earl of* Morton. *P. R. S.*

My Lord,

Read May 5, 1768.

THE incloſed account of inoculation in the Eaſt, I have juſt received from my brother at Aleppo, and though nothing farther ſeems wanting in this country to remove prejudices againſt that practice, yet I thought its being made public might be of ſome uſe to other European nations, where ſuch prejudices ſtill prevail; and as a matter of curioſity, would not be unacceptable to the Royal Society. I have therefore taken the liberty to trouble your Lordſhip with it for that purpoſe.

Juſt before my leaving Aleppo, I did hear that it was practiſed amongſt ſome of the Bedouins there, and went by the name of buying the ſmall pox; but being then much engaged with other buſineſs, it quite eſcaped my memory, and indeed my information was ſo ſlight, that I did not think it right to mention it in my Natural Hiſtory of Aleppo.

I ſhall

I ſhall only add, that my brother has been more prolix in the narrative than perhaps was neceſſary, had the facts come within his own knowledge; but ſo far as depended upon the intelligence of others, he thought it beſt to explain the foundation of his own belief.

I have the honour to be,

My Lord,

Your Lordſhip's

moſt obedient ſervant,

Walbrook, April 18, 1768.

Alexander Ruſſell.

Dear Brother,

FROM the manner in which inoculation is mentioned in the Natural Hiſtory of Aleppo, I ſuſpect the circumſtance of it's being a common practice among the Arabs muſt have eſcaped you. I myſelf was ignorant of it for ſeveral years after you left this country, and a mere accident brought it at laſt to my knowledge. About nine or ten years ago, while on a viſit at a Turkiſh Harem, a lady happened to expreſs much anxiety for an only child, who had not yet had the ſmall pox; the diſtemper at that time being frequent in the city. None of the ladies in the company had ever heard of inoculation; ſo that, having once mentioned it, I found myſelf obliged to enter into a detail of the operation, and of the peculiar advantages attending it. Among the female ſervants in the chamber was an old Bedouin, who having heard me with great attention, aſſured the ladies, that my account was upon the whole a juſt one, only that I did not ſeem ſo well to underſtand the way of performing the operation, which ſhe aſſerted ſhould be done not with a lancet, but with a needle; ſhe herſelf had received the diſeaſe in that manner, when a child; had in her time inoculated many; adding moreover, that the practice was well known to the Arabs, and that they termed it buying the ſmall pox.

In consequence of this hint, I set about the procuring more particular information from the Arabs of this place; and the result of my enquiry was, that the practice of inoculation had been of long standing among them. They indeed did not pretend to assign any period to its origin; but those of seventy years old and upwards remembered to have heard it spoken of as a common custom of their ancestors, and made little doubt of its being of as ancient a date as the disease itself. Their manner of operating is, to make several punctures in some fleshy part, with a needle imbued in variolous matter, taken from a favourable kind of pock. They use no preparation of the body; and the disease communicated in this way being, as they aver, always slight, they give themselves little or no trouble about the child in the subsequent stages of the distemper.

This method of procuring the disease is termed, buying the small pox, on the following account. The child to be inoculated carries a few raisins, dates, sugar plumbs, or such like, and shewing them to the child from whom the matter is to be taken, asks how many pocks he will give in exchange. The bargain being made, they proceed to the operation. When the parties are too young to speak for themselves, the bargain is made by the mothers. This ceremony, which is still practised, points out a reason for the name given to inoculation by the Arabs; but, by what I could learn among the women, it is not regarded as indispensably necessary to the success of the operation, and is in fact often omitted.

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The Bedouins at this place, who are employed in the ſervice of the Harems, more rarely have recourſe to inoculation, their children being often brought up in company with thoſe of the Turks, by whom, as you juſtly obſerve, the practice is not admitted. But the Bedouins, leſs connected with the Turks, who dwell within the city; thoſe who live in tents without the city walls, and the Arabs of the adjacent deſart under the Emir, do commonly inoculate their children.

It being highly probable that a practice, which was ſo common in theſe parts, might be known alſo to the more Eaſtern Arabs, I applied for information to ſeveral Turkiſh merchants of Bagdat and Mouſul, who occaſionally reſide a few months in the year at Aleppo. By thoſe I was aſſured, that inoculation was not only common in both the cities firſt mentioned, but alſo at Baſſora; and that at Mouſul particularly, when the ſmall pox firſt appeared in any diſtrict of the city, it was a cuſtom ſometimes to give notice by a public crier, in order that ſuch as were inclined might take the opportunity to have their children inoculated.

I enquired at the ſame time of the Bagdat merchants, whether the Arabs, who dwell on the banks of the river between that city and Baſſora, uſed the ſame method of propagating the ſmall pox. They told me, they believed it to be common alſo among thoſe Arabs; though (with an ingenuity not uſual in this country) they owned they had never thought of enquiring about the matter, and might therefore perhaps be miſtaken. But I afterwards had

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an opportunity of being better informed by the Arabs, who come hither with the Eaſtern caravans; from whoſe-accounts it would appear, that inoculation has from time immemorial been a practice among the different Arab tribes with which they were converſant; comprehending, beſides thoſe in the numerous encampments on the banks of the Euphrates, and the Tigris below Bagdat, other tribes in the vicinity of Baſſora, and in the deſart.

For theſe ſeveral years paſt, very few ſlaves have been brought from Georgia. From what I could collect among thoſe already here, who remember any thing of their own country, inoculation was well known there: I have ſeen ſeveral old Georgian women, who had been inoculated, when children, in their fathers houſes.

In Armenia, the Turkoman tribes, as well as the Armenian Chriſtians, have practiſed inoculation ſince the memory of man; but, like the Arabs, are able to give no account of its firſt introduction among them.

To what extent inoculation reaches in the Gourdeen mountains, I do not know with any certainty: it is practiſed by the Gourdeens in the Mountains of Bylan, and Kittis; and, I have reaſon to think, extends much further.

At Damaſcus, and all along the coaſt of Syria and Paleſtine, inoculation has been long known. In the Caſtravan mountains it is adopted by the Druſi, as well as the Chriſtians.

Whether the Arabs of the deſart, to the South of Damaſcus, are acquainted with this manner of com-

municating the ſmall pox, I have not hitherto been able to learn; but a native of Mecca, whom I had occaſion to converſe with this ſummer, aſſured me, that he himſelf had been inoculated in that city.

It has already been mentioned, that the Turks at Bagdat and Mouſul make no ſcruple to inoculate their children. I have ſeen alſo ſome Turkiſh ſtrangers here, who had been inoculated at Erzeroon. Hence it is probable that the Turks, in other parts of the Ottoman empire, do not merely, as fataliſts, reject inoculation; but that other conſiderations, which have influence in countries where fataliſts are ridiculed or anathematized, concur likewiſe in Turky, to oppoſe the reception of a practice ſo beneficial to mankind. The child of a Baſhaw here, was by my advice inoculated about eight years ago; but that is the only inſtance I have known among the Turks at Aleppo.

The Jews at this place abſolutely reject inoculation; partly from ſcruples of a religious kind, and partly from the diſtruſt of its ſucceſs. At Bagdat, Baſſora, and in Paleſtine, having acquired a more favourable opinion of an operation which they ſee ſo often performed with ſucceſs, they have got the better of other ſcruples, and join in the practice with their neighbours.

I have ſeveral times converſed on this ſubject with the Mufti here, as alſo with ſome of the Rabbis; but the theology of both was too abſtruſe for me: their arguments, ſo far as I was able to comprehend them, ſeemed to be no leſs cogent againſt all chirurgical operations, which were attended with

any

any degree of danger to life, than againſt inoculation.

In the different countries above-mentioned, inoculation is performed nearly in the ſame manner. The Arabs affirmed, that the punctures might be made indifferently in any fleſhy part: thoſe I have had occaſion to examine, have all (a very few excepted) had the mark between the thumb and the forefinger.

Some of the Georgians had been inoculated in the ſame part, but moſt of them on the fore-arm. Of the Armenians ſome had been inoculated in both thighs; but the greateſt part (like the Arabs) bore the marks upon the hand. Some of the Georgian women remembered, that rags of a red colour were choſen in preference for the binding up the arm, a circumſtance of which I have been able to diſcover no trace among the Arabs.

Buying the ſmall pox, is likewiſe the name univerſally applied to the method of procuring the diſeaſe. There are, it is true, other terms made uſe of both in the Arabic and Turkiſh Languages; and at this place, it is principally known to the Chriſtians by the name of inoculation.

From the ſameneſs of the name, as well as from the little diverſity obſervable in the manner of performing the operation, it is probable the practice of inoculation in theſe countries was originally derived from the ſame ſource: and that it is of conſiderable antiquity, can hardly be doubted, if we conſider the large extent of country over which it is found to have ſpread, and the obſtacles it muſt have met

with in a progreſs through various nations, of which ſome are ſeparated by polity as well as religion, while others, peculiarly tenacious of their own cuſtoms, are little diſpoſed to admit thoſe of ſtrangers.

That no mention is made of inoculation by Rhazes, Avicenna, or any other of the ancient Arabian medical writers known in Europe, is, I believe, in general ſuppoſed; and I am aſſured by the native phyſicians here, that nothing is to be found regarding it, in any of a more modern date. Some learned Turkiſh friends here, ſome time ago were prevailed on at my requeſt to make enquiry, but have not hitherto been able to diſcover any thing concerning inoculation; although they ſearched not only the medical writers, but alſo the hiſtorians, and ſome of the poets.

It appears from accounts communicated to the Royal Society, in the year 1723, by Doctor Williams and Mr. Wright, that inoculation had been known in certain parts of Wales ſo far back as the laſt century; and it is remarkable, that it there bore the ſame name, by which it is moſt generally known to the Arabs. I think it has alſo been diſcovered to be an ancient practice among the vulgar in different parts of the continent.

If inoculation was really known ſo long ago in Europe, and the accounts of it till within theſe fifty or ſixty years are found to be merely traditional, the ſilence of the Arabian writers, on a practice which probably was never adopted by their phyſicians, is the leſs to be wondered at. What may perhaps appear more ſtrange, is, that after the year 1720, though the

the curiosity of the public has, at different times, been excited by the controversies relating to inoculation, the state of that practice in Syria, where there were so many European settlements, should have remained unknown both in England and in France, which probably was the case, as the advocates for inoculation have made no reference to it.

Whether before the account transmitted by Pylarini to the Royal Society, inoculation had not been mentioned by any of the travellers who had visited these countries, I do not presume to determine. In the books I have had occasion to peruse, there is nothing to be found on the subject. Among the travellers the most likely to have mentioned it was Rauwolf: yet, however rational it may be to think that a practice of such a kind, had it then prevailed, could hardly have escaped the notice of so diligent an observer, it would be rash to infer from his silence that it was not known to the Arabs in the sixteenth century. The justly celebrated French botanist is equally silent, though in the beginning of the present century he visited several places where inoculation was undoubtedly at that time both known and practised.

Having related in what manner I came to learn inoculation was known to the Arabs, I can arrogate no merit in the discovery; nor would I be thought to insinuate any reflection on the accuracy of the indefatigable M. Tournefort, to whose labours the curious stand so much indebted. Customs the most common, in distant countries, are often of all others the

the leaſt apt to attract the obſervation of travellers, who, engaged in other purſuit, muſt be indebted to accident for the knowledge of ſuch things, as the natives ſeldom talk of, from the belief that they are known to all the world. This conſideration may, in ſome meaſure, account for inoculation having been over-looked by thoſe who have tranſiently paſſed through theſe countries; and is all we can offer as an apology, for the having remained ſo long unacquainted with a fact in medical hiſtory, in a ſituation where we both had ſo many opporunities of information. I am,

Dear brother,

Moſt affectionately yours,

Aleppo, Nov. 26, 1767.

P. Ruſſel.

Received May 5, 1768.

XXI. *Part of a Letter from Dr.* Wolfe, *at* Warſaw, *to* Henry Baker, *F. R. S. Communicated by Mr.* Baker.

Warſaw, April 15, 1768.

Read May 12, 1768.

OUR winter has been very long and very ſevere. The froſt was at a medium, from the middle of December to the middle of March, at nine degrees of Reaumur's thermometer below the freezing point. Three times it was from 19 to 20 degrees, and very ſeldom aſcended to the freezing point. Laſt year the froſt was at 24 degrees, but of no duration. In the year 1740 it was at 26 degrees, which, perhaps, is the loweſt it has been remembered in this country. This year we have had little or no ſnow, which made the froſt exceedingly more ſenſible. The barometer is here, at a medium, at 28 $\frac{1}{3}$ inches Rhinland meaſure: great ſtorms ſink it two inches lower; and very dry weather raiſes it 1 $\frac{1}{3}$ inch higher. The declination of the needle at this place is eleven degrees and half to the weſt.

XXII. *Extract*

XXII. *Extract of a Letter from Mr.* Peter Wargentin, *Secretary of the Royal Academy of Sciences at* Stockholm, *and F. R. S. dated* February 23, 1768, *to the Reverend* Nevil Maſkelyne, *B. D. F. R. S. Aſtronomer Royal.*

Read May 12, 1768.

AS it is related in the public newſpapers, that the weather * has been uncommonly cold in Germany, England, and France, towards the end of laſt year, and the beginning of the preſent year, I have here ſent you the degrees of altitude by Reaumur's thermometer, as I have obſerved them ſince the beginning of November 1767. The altitudes here ſet down are the arithmetical means of three taken every day, viz. in the morning before ſun-riſe, a little after noon, and at ten o'clock at night. Hence it appears that the cold has been moderate here with reſpect to this climate, and nothing more than common, though it was without intermiſſion, from the beginning of December. The greateſt cold in theſe months happened on the ſecond day of January in the morning; the quick-ſilver then ſtood at 17,2 below the point of freezing.

* The leaſt height of Fahrenheit's thermometer, ſet down in the courſe of the aſtronomical obſervations made at the Royal Obſervatory, was 15° on Jan. 6, at the tranſit of Venus over the meridian at 8 h. 42. m. A. M. At which time nearly the ſame was obſerved by my Lord Charles Cavendiſh in Great Malbro' Street, at 17°. But by a thermometer deſcribed in Vol. L. of theſe Tranſactions, placed on the top of the ſame houſe, in a very bleak ſituation, Dec. 31. it appeared to have been at 12 $\frac{1}{2}$° in the preceding night; Jan 3. 16°; Jan. 6. 16°, and Jan. 7. 13°. N. M.

Day

Day of the M.	Nov.	Dec.	Jan.	Feb.
1	6,3 +	4,2 —	13,3 —	2,3 —
2	5,2 +	6,4 —	14,1 —	6,1 —
3	4,5 +	4,3 —	8,2 —	4,1 —
4	2,4 +	4,4 —	4,3 —	3,5 —
5	2,2 +	6,8 —	9,7 —	5,5 —
6	1,9 +	8,6 —	6,0 —	0,4 —
7	1,0 +	0,7 +	4,0 —	3,7 —
8	3,5 +	0,6 —	9,8 —	0,4 —
9	6,3 +	5,0 —	7,6 —	0,4 —
10	7,5 +	5,2 —	11,2 —	2,6 —
11	5,9 +	0,2 +	7,2 —	1,4 +
12	4,6 +	2,0 +	5,8 —	2,7 —
13	6,0 +	3,4 —	6,0 —	5,8 —
14	3,7 +	1,8 +	1,7 —	6,6 —
15	5,5 +	3,8 +	0,4 —	4,0 —
16	4,0 +	4,6 +	1,0 —	6,8 —
17	5,6 +	3,4 +	0,9 —	6,9 —
18	5,3 +	0,8 +	1,2 —	5,9 —
19	2,4 +	0,6 +	1,3 —	3,2 —
20	2,6 +	1,6 —	2,1 —	1,8 —
21	4,3 +	2,5 —	3,5 —	2,5 —
22	4,4 +	2,5 —	1,5 —	3,1 —
23	3,0 +	4,5 —	1,0 —	
24	0,2 —	1,3 —	0,3 +	
25	1,8 +	1,6 —	0,2 —	
26	1,7 +	2,2 —	0,4 +	
27	2,4 +	3,0 —	0,8 +	
28	1,0 +	0,4 —	1,1 +	
29	0,5 —	2,7 —	1,0 +	
30	0,2 —	7,0 —	1,5 —	
31		9,6 —	1,0 —	

XXIII. *Introduction to two Papers of Mr.* John Smeaton, *F. R. S. by the Reverend* Nevil Maſkelyne, *B. D. F. R. S. Aſtronomer Royal.*

Read May 12, 1768.

THE two following papers I received from my ingenious, and much eſteemed friend, Mr. John Smeaton, with his deſire, that I ſhould communicate them to the Royal Society, if I thought they contained any hints conducive to the improvement of aſtronomy. As the firſt paper points out the time of obſerving the menſtrual parallaxes of the planets in thoſe circumſtances in which they will be greateſt, and at the ſame time ſhews how to obviate the error, which would otherwiſe ariſe from the inaccuracy of their theories (which muſt neceſſatily be uſed in the calculation), by correcting them from other obſervations, made on purpoſe, before and after the firſt mentioned obſervations; and the ſecond paper gives a new and accurate method of obſerving the places of the heavenly bodies out of the meridian, independent of refraction, I apprehend they will prove acceptable preſents to aſtronomers.

I ſhall only add one other remark, that has been ſuggeſted to me from the peruſal of Mr. Smeaton's firſt paper; that, as it is there propoſed to find the dimenſions of the orbit deſcribed by the revolution of the Earth about its common centre of gravity and the Moon's, by means of the menſtrual parallax of Mars, near his oppoſition, or of Venus, near her conjunction

 with

with the Sun; the ſame may alſo be determined with advantage from the ſum of the menſtrual parallaxes of theſe two planets, when they happen to be in the required poſitions at the ſame time, which will indeed happen but ſeldom; or even from the ſum of the menſtrual parallaxes of Mars and the Sun, which may be obſerved together at every oppoſition of Mars to the Sun; the ſum of the menſtrual parallaxes of Mars and Venus in theſe circumſtances, according to the numbers uſed by Mr. Smeaton, will ſometimes amount to 87″, and the ſum of thoſe of Mars and the Sun to 52″.

Nevil Maſkelyne.

XXIV. *A Discourse concerning the Menstrual Parallax, arising from the mutual Gravitation of the Earth and Moon; it's Influence on the Observations of the Sun and Planets; with a Method of observing it: By* J. Smeaton, *F. R. S.*

Read May 12, 1768.

IT is demonstrated by Sir Isaac Newton in the *Principia*, that it is not the Earth's center, but the *common center of gravity* of the Earth and Moon, that describes the ecliptic; and that the Earth and Moon revolve in similar ellipses, about their common center of gravity. The same great author has also investigated, from the different rise of the tides, when the Moon is in conjunction or opposition to the Sun, to those which happen when the Moon is in her quadratures; that the quantity of matter in the Earth is to that in the Moon, as 39.78 to 1; from whence, and the known distance of the Earth and Moon, it would follow, that the common center of gravity of the two bodies falls without the surface of the Earth, by one half of its semidiameter: that is, that the center of the Earth describes an epicycle round the common center of gravity once a month, whose diameter is three semidiameters of the Earth.

Dr. Gregory, in his astronomy, has laid hold of this circumstance, in order to prove the relative gravity of the Earth and Moon, by observation; which is the subject of his 60th proposition of the fourth book;

book; in which he has demonſtrated, that if an obſerver on the Earth makes a correct obſervation on the Sun's place, when the Moon is in one quadrature, it will differ from a like obſervation, taken in the oppoſite quadrature (according to a mean elliptic motion) by an angle which the diameter of this epicycle will ſubtend at the Sun. The ſame learned author has alſo ſhewn, in the ſcholium to the ſame propoſition, that this quantity, or parallax, will be twice greater to Mars in oppoſition, and three times greater to Venus, in her inferior conjunction with the Sun.

The difference thus produced in the apparent place of the Sun, and of all the primary planets being governed by the Moon, and having it's period the ſame, may perhaps be not unaptly called the *menſtrual parallax*.

Now if, with Sir Iſaac Newton, the relative gravities of the Earth and Moon are taken between the proportion of 39 and 40 to one; the menſtrual parallax of the ſun will come out 13″ upon the radius of the Earth's epicycle, and will affect the ſolar obſervations at the oppoſite quadratures, by double that quantity, viz. 26″: in like manner, the mean diſtance from the Earth of Mars in oppoſition, being to the Sun's mean diſtance, as 1 to 2. 1; and the leaſt diſtance of Mars from the Earth, to the Sun's mean diſtance, as 1 to 2 $\frac{1}{4}$, the menſtrual parallax of Mars will affect the obſervation upon him in that ſituation, by 56″ and 73″ $\frac{1}{2}$, reſpectively.

The mean diſtance of Venus from the Earth, in her inferior conjunction, being to that of the Sun as 3 $\frac{1}{2}$ to 1 nearly, and not very variable, on account

of

of the orbit of Venus being almoſt circular; the menſtrual parallax would affect the place of Venus, in that ſituation, by a quantity not leſs than 92″; and in all other ſituations in proportion to her diſtance; which alſo holds with reſpect to all the reſt of the planets.

Theſe diſturbing quantities are by no means to be diſpenſed with, in the nice and critical ſtate that aſtronomical obſervations and calculations have arrived at, in conſequence of the diſcoveries of Dr. Bradley, who may be ſaid to have given a baſis to aſtronomy; however, could we rely upon the *data*, on which Sir Iſaac's inveſtigation of the relative gravity of the Earth and Moon is founded, we ſhould have nothing to do but to apply an equation to the particular caſes, according to the diameter of the epicycle, as deduced from the relative gravity; but whoever conſiders the great obſtructions that the water of the ſea meets with in its motion to obey the influence of the Moon; the great difficulty in aſcertaining the true height of the tides, from the many diſturbing cauſes intervening; and the many uncertainties, and want of coincidence, that have attended, and muſt attend, ſuch obſervations; muſt confeſs, that this matter does not ſeem capable of ſuch a determination from that quarter, as the preſent ſtate of aſtronomy requires.

Accordingly, ſince the time of Dr. Gregory, thoſe great aſtronomers Dr. Bradley, De la Caille, and others, have applied themſelves to determine the quantity of the menſtrual parallax from ſolar obſervations: but though theſe have given cauſe to ſuppoſe that the relative gravity of the Earth and Moon are not above $\frac{2}{3}$ of the quantity deduced from the tides; yet, as the obſervation

obſervation of theſe ſmall angles principally depends upon the obſervation of the Sun's right aſcenſion (which, depending on the meaſure of time, is leſs capable of exact obſervation, than if depending on divided inſtruments); the deductions thence drawn ſeem ſtill wanting of that certainty which the ſubject demands; and if to this we add, from a deduction of Mr. Maſkelyne, that the relative gravity of the Earth and Moon is as 76 to 1, derived from the effect that the Moon produces in the nutation of the Earth's axis; the relative gravity, and conſequently the parallaxes thereon depending, will be reduced to almoſt one half of thoſe reſulting from Sir Iſaac's determination.

It is true, that the quantity of effect of the menſtrual parallaxes will not be great, if computed upon Mr. Maſkelyne's induction, for as much as that the common center of gravity will be conſiderably within the Earth's ſurface; yet, even in that caſe, the Sun's tranſit over the meridian, when the Moon is in one quadrature, will differ nearly one ſecond of time from that obſerved in the oppoſite quadrature; and though De la Caille and Mayer have formed equations depending on the Moon, to be applied to the equation of time; yet, if we are at an uncertainty, whether the *maximum* of this equation is one ſecond, two thirds of a ſecond, or half a ſecond of time, each way, we are ſtill under a material difficulty; for though theſe differences are ſo ſmall, that it is not eaſy to determine them exactly from ſolar obſervations; yet, as they are capable of creating a ſenſible difference in theſe obſervations, they will, ſo long as they remain undetermined, prevent that ſolidity and firmneſs

neſs to the ſolar obſervations, which is the more neceſſary as they are the foundation of all the reſt: but with reſpect to thoſe planets, that in their periods come nearer to us than we to the Sun, the obſervations upon them will be affected by a greater uncertainty.

The determination of the menſtrual parallax is of ſtill more importance, as it is a neceſſary conſideration in the determination of the Sun's parallax; and this, whether deduced from Mars or Venus, as I ſhall preſently ſhew more particularly; but firſt I muſt ſtate the quantity of the menſtrual parallax, according to the beſt *data* yet known, by a contrary proceſs; and, taking the mean quantity of the Sun's parallax, according to the determination of Mr. Short, at 8″ 8, and the relative gravities of the Earth and Moon, according to Mr. Maſkelyne, as 76 to 1, and the mean diſtance of their centers equal to 60 $\frac{1}{2}$ ſemidiameters; we ſhall then have the diſtance of the Earth's center from the center of gravity, at $\frac{8}{10}$ of the Earth's ſemidiameter (that is, $\frac{1}{5}$ of that ſemidiameter within the Earth's ſurface) and the menſtrual parallax equal to $\frac{8}{10}$ of the Sun's parallax; conſequently about 7″; and the double menſtrual parallax, or vacillation, ariſing from the whole diameter of the epicycle, 14″; the mean menſtrual parallax of Mars in oppoſition, 29″ $\frac{1}{2}$; the greateſt, 38″ $\frac{1}{2}$; and that of Venus 49″; from hence it follows, that, was a perſon to attempt the Sun's parallax, by the diurnal motion of the Earth, applied as a baſis to Mars in oppoſition, as has formerly been tried; and ſhould the Moon be at new or full at the ſame time, the change of place of the Earth's center, in its own epicycle, would amount to an angle ſeen from Mars of

of $1''.3$ nearly; that is, in caſe the interval between the obſervations was eight hours, and Mars at his mean diſtance; but if Mars was not at his neareſt diſtance, this change would in the ſame time amount to $1''.7$ nearly. In like manner, if a tranſit of Venus happens near the new or full Moon (as will be the caſe next year), the time of the tranſit will be affected by a change of place, ſuch as the Earth's center will deſcribe in its epicycle, during the time of the whole tranſit, if the beginning and end are obſerved in the ſame place; or during the difference of abſolute time, at which the tranſit appears to begin or end to different obſervers in diſtant meridians. Thus, when the ſame obſerver ſees the beginning and end in the ſame place, the baſe deſcribed by that obſerver, from the Earth's diurnal motion, muſt be corrected by the ſpace, deſcribed by the Earth's center, in the circumference of it's epicycle, during that time; which, if it be ſuppoſed of ſeven hours, will amount to an angle of $1''.9$, ſeen from Venus: but, where the beginning or end is ſeen by different obſervers in diſtant meridians, as the difference of abſolute time can hardly amount to above 15 minutes, the change of place of the Earth's center will for that time be but ſmall; however, at the rate beforementioned, it will for 15 minutes affect the parallactic angle ſeen from Venus, by about $\frac{7}{100}$ of a ſecond; and the parallax of the Sun, by about $\frac{1}{400}$ part of the whole: but this proportional part will remain the ſame, whether the diſtance of meridians be ſuch as produce a greater or leſs difference of abſolute time than 15 minutes*.

* If an error of $\frac{1}{400}$ part of the whole may be ſuppoſed in the obſervation for determining the Sun's parallax by the tranſit

From what has been said, I suppose it will appear, that the effects of the menstrual parallax are worthy of consideration; and that nothing has been yet executed, whereby it has received a determination sufficiently accurate; for, in regard to observations upon the Sun, the whole quantity is too small to be minutely observed in right ascension: and with respect to the application to Mars and Venus, as suggested by Dr. Gregory, I do not know that any thing has been done; and indeed no wonder, as the theory of the motion of Mars and Venus has not been as yet so critically reduced to computation, as to render their parallaxes (though in themselves much greater) deducible with equal certainty as that of the Sun.

What I therefore have now to propose, is a method of observing the menstrual parallaxes of Mars and Venus, without laying any undue stress upon the theory of their motions.

The first opportunity of making an observation for this purpose, will be at the next opposition of Mars; which, according to the Nautical Almanack, will happen the 26th of October next, in the morning; I will therefore endeavour to illustrate this matter by taking that as an example.

The distance of Mars from the Earth will then be somewhat less than the mean distance, that is, as 1 to 2.2; and consequently his double menstrual parallax, according to Mr. Maskelyne, will be near 31″ in the point of opposition. Now, as the Moon

of Venus, a neglect of the menstrual parallax may make it [illegible] part of the whole.

will

will be at full, not above 12 hours preceding that opposition, the Moon will be nearly in the most favourable situation for the purpose.

For this end, let an accurate observation be made upon the place of Mars at the following times, viz. first, near the time of the new Moon, preceding Mars's opposition; or more properly at the nearest opportunity, to the time of the Moon's opposition to Mars; which will happen in the night, between the 12th and 13th of October: secondly, let the place of Mars be observed when the Moon is nearest her quartile with Mars; that is, between the 19th and 20th of the same month: thirdly, let an observation on Mars be made when the Moon is in conjunction with Mars, the nearest to his opposition with the Sun; that is, between the 25th and 26th of ditto: fourthly, let Mars again be observed when the Moon has moved on to her quartile with Mars, viz. between the 31st of October, and 1st of November: and fifthly and lastly, let the place of Mars be observed, when the Moon has again got to her opposition with Mars, which happens between the 7th and 8th of November.

Now it is manifest, that, when the Moon is in conjunction or opposition to Mars, the center of the Earth, the center of Mars, and the common center of gravity of the Earth and Mars, will be nearly in a right line, and consequently, that an observer will then see Mars, in the same place in the heavens, as if the common center of gravity was the same as the center of the Earth; therefore, then the place of Mars will be unaffected by a menstrual parallax; and such will be the first, third, and fifth of the observations above propounded.

It is equally evident, that when the Moon is in quartile with Mars, and moving towards a conjunction, an obſerver, at the Earth's center, will ſee Mars more backward in the ecliptic, than if ſeen from the common center of gravity, by $15'' \frac{1}{2}$; and that, when the Moon is in her oppoſite quartile with Mars, and moving from her conjunction, that then an obſerver at the Earth's center, will ſee Mars advanced in his orbit more forward by $15'' \frac{1}{2}$, than if ſeen from the common center of gravity; and the one obſervation checqued with the other, will, according to a mean elliptic motion, differ by the quantity of $31''$; and ſuch will be the ſecond and fourth obſervations above propounded.

Now, from the firſt, third, and fifth, obſervations, three points of Mars's orbit will be given; which, by the help of the theory of Mars's motion in an elliptic orbit, whoſe aphelion, eccentricity, and nodes, are known ſufficiently near for this purpoſe; the intermediate places of Mars may be inferred with the requiſite degree of accuracy: and particularly, as the two intermediate obſervations, viz. the ſecond and fourth, will be nearly at equal intervals of time between the three others: from hence it follows, that the difference between the inferred, or computed places, at the quartiles, and the obſerved places at thoſe times, will be the menſtrual parallax required.

It is to be noted, that the times above ſpecified are the moſt favourable for the obſervation; and could thoſe be made uninterruptedly from weather, there would be the leſs occaſion for any other: but, as much as poſſible to prevent diſappointments of this kind, it will be right to begin the obſervations, a month preceding

ing making the proper obſervations, at the conjunctions, quartiles, and oppoſitions, of the Moon with Mars, which will be the means of ſupplying ſuch obſervations, as may happen to prove abortive before the oppoſition of Mars, and alſo, in caſe any of the obſervations to be made after that oppoſition ſhall prove deficient, the obſervations may be carried on for a month or competent time afterwards. As a further ſecurity againſt diſappointments, as well as cheque, it will alſo be adviſable to make the proper obſervations, the night preceeding and ſubſequent to thoſe in which the quartiles, conjunctions, &c. happen; for, as the quantities will not differ conſiderably from thoſe obtained on the days ſpecified, with proper allowances they may be brought in ſupport and confirmation of the former.

In like manner, when Venus is moving towards her inferior conjunction with the Sun, as will happen next year, the ſame obſervations may be made with reſpect to her; and continued for a neceſſary time, to get obſervations of the place of Venus; viz. the firſt, when the Moon is in conjunction or oppoſition with Venus: a ſecond, when the moon is in her quartile with Venus: a third, in conjunction or oppoſition: a fourth, when the moon is in her oppoſite quartile to the former: and a fifth, again in conjunction or oppoſition: the ſame opportunity will alſo offer when Venus is moving from her inferior conjunction with the Sun, and becomes a morning ſtar.

In regard to the obſervation of Venus, it is remarked by aſtronomers, that ſhe is to be ſeen with a good tranſit teleſcope, when ſhe is within a few degrees of the Sun; but, as ſhe is three times nearer the Earth, than

than the Sun's mean diſtance, when her elongation is 25° in the inferior part of her orbit, it is plain, that the neceſſary obſervations may eaſily be made, when her menſtrual parallax will be at a medium, three times greater than the Sun's; and conſequently amounting for the whole difference to 42″.

To avoid embarraſſment in deſcription, I have hitherto ſuppoſed, that all the obſervations are made in the meridian; in which caſe the right aſcenſions will be the ſame as they would appear from the center of the Earth; and conſequently, the planet's longitudes thence deduced, nearly the ſame: but 'tis eaſy to ſee, that if the quartile obſervations are made when the planets are conſiderably to the eaſt or weſt of the meridian, and ſo choſen, that the place of the obſerver be further diſtant from the common center of gravity, than the center of the Earth is from that center, that the baſe of the obſervations will be conſiderably enlarged. Thus, in our latitude, ſuppoſing that the quartile obſervations are made four hours before and four hours after the planet paſſes the meridian, this will produce an enlargement of the baſis by one of the Earth's ſemidiameters: and as the whole baſe or diameter of the epicycle comes out, according to Mr. Maſkelyne, no more than 1.6 of the Earth's ſemidiamers; the baſe will, according to this method, come out 2.6; and conſequently, at the next oppoſition, the menſtrual parallax of Mars will be thereby enlarged to 50″, the greateſt to 62″ $\frac{1}{2}$, and that of Venus at a mean, to 74″ $\frac{1}{2}$.

It muſt however be acknowledged, that no kind of obſervations of the places of the planets are of equal validity with thoſe taken with the beſt inſtruments in the

the meridian; thoſe taken with micrometers perhaps not excepted: for however accurately ſmall diſtances can be meaſured by the micrometer of Mr. Dollond, yet, as theſe meaſures can hardly be reduced to the ecliptic, without having the difference of declination or right aſcenſion from other means (except two ſtars making ſomewhat near a right angle with the planet ſhould appear within the field of view at once); and as in all theſe caſes the rectification of the places of the ſtars themſelves ultimately depends on meridian obſervations we may perhaps be allowed to ſay, that in the moſt favourable caſes of the micrometer, the determinations thence to be drawn, are not ſuperior to meridian obſervations, and in leſs favourable caſes, muſt be inferior: however, as the micrometer obſervations out of the meridian give an opportunity of repetition as often as we pleaſe; and the obſervations for rectification of the ſtars concerned, can be repeated in the meridian, as often as we pleaſe alſo; it muſt be equally allowed, that when theſe kind of obſervations are taken, not too near the horizon, when proper ſtars offer for this purpoſe, and the whole ſkilfully managed; theſe kind of obſervations fall but little ſhort of thoſe taken immediately in the meridian. I cannot therefore heſitate to recommend, that the quartile obſervations be taken out of the meridian, as well as in it: in the firſt place, by Dollond's micrometer, if ſtars offer in proper poſitions; and if not, ſecondly, by taking differences of right aſcenſion and declination between the planet and the ſtars, by the common micrometer, in caſe proper ſtars offer themſelves for this pupoſe: but as it frequently happens, that no proper ſtars offer themſelves to micrometers

of either kind; and this is ſtill more likely to happen in the obſervations of Venus, which will be chiefly in the day light; I beg leave to offer (what to me is) a new method of obſervation out of the meridian; and which, though I eſteem it not equal to micrometer obſervations of either kind, I apprehend will fall ſo little ſhort thereof, and prove ſo much ſuperior to any other method now in practice in theſe caſes, that I hope I ſhall on this occaſion be excuſed, in giving a particular deſcription thereof: but, as it is a general method of obſerving out of the meridian, I ſhall reſerve it by way of appendix.

In the next obſervation of Mars, it has been ſtated, that, in the meridian obſervations alone, the menſtrual parallax, according to the ſmalleſt eſtimation, may be expected to amount to 31″ in longitude; which, turned into right aſcenſion, will make about 2″ of time: now, if it may be allowed, that a well-practiſed obſerver can take the time of a tranſit to $\frac{1}{4}$ part of a ſecond, over a ſingle wire, if he has three wires, or more, as uſual, the mean of the three ſhould be within $\frac{1}{12}$ part of a ſecond; or within $\frac{1}{24}$ part of the whole quantity in queſtion: it is however a matter of chance, whether the mean of three may or may not be within $\frac{1}{5}$ part of the whole; and as equal errors may be committed in the obſervations of the tranſits of the ſtars, wherewith the right aſcenſions of the planets in queſtion are compared; which it is an equal chance, whether they tend to correct or increaſe the errors committed in the former; yet if, as has already been propoſed, the obſervations are continued for two or three months, instead of one; and obſervations taken the day preceding and ſubſequent

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to the days of conjunction, quartile, and opposition; and this as well out of the meridian as in it; we can hardly doubt but that, if the weather should favour, so many cheques would be formed, that, from the next opposition of Mars alone, the affair may be brought within a 24th part of the whole; and, if to this be added the force of such determinations, as may be drawn from observations on Venus, before and after her transit over the Sun next year, it can hardly be doubted, but that those three will bring us within a single second of a degree, subtended from the nearest planet; and these conclusions will be further strengthened by future observations; as two years will scarcely pass without affording one or more opportunities of this kind.

As I meant not to embarrass myself with exact computations, I have constantly supposed the distance of the common center of gravity from the center of the Earth, to be a fixed quantity; whereas it will vary in the same proportion as the Moon's distance varies; but, as this and many other *minutiæ* will properly enter the computation, when the observations are made, I must beg leave to refer them to the learned in this science.

Austhorpe, April 17, 1768.

J. Smeaton.

XXV. *Description of a new Method of observing the heavenly Bodies out of the Meridian: By* J. Smeaton, *F. R. S.*

Read May 16, 1768.

THE inſtrument I propoſe for this purpoſe, is a tranſit teleſcope, mounted on a vertical axis; for example, ſuch a one as is deſcribed in the introduction to the *Hiſtoire Celeſte* of Mr. le Monnier; being one of the inſtruments made by Mr. Graham for the academicians who went to meaſure a degree at the Polar circle; this or any other inſtrument upon equivalent principles will ſuffice, that is, capable of ſuch adjuſtments, as to be made correctly to deſcribe an almicanther and azimuth circle; and capable of being retained in any given poſition: the uſe will appear by the following example.

Make choice of any fixed ſtar, which according to the diurnal motion, precedes the heavenly body to be obſerved by a few minutes, more or leſs, as it may happen; let the inſtrument be ſet to an azimuth, ſomewhat preceding the fixed ſtar; and carefully obſerve the time of the ſtar's tranſit croſs the vertical wire of the teleſcope; then wait till the heavenly body comes to the ſame azimuth; and, when arrived within the field of view, keep gently turning the ſcrew that alters the elevation of the teleſcope, ſo as to follow the heavenly body in altitude; keeping it interſected

by the horizontal wire of the telefcope, till the body paffes the middle vertical wire, and carefully note the time of its paffage; there leave the telefcope fixed as to altitude, and releafing the horizontal motion, turn it round on it's vertical axis, till you meet with fome ftar, that in a little time after will by rifing or falling come to the fame almicanther; and, on it's arrival, carefully note the time of it's paffage crofs the horizontal hair of the telefcope.

Now, from the right afcenfions and declinations of the two ftars being previoufly known, or afterwards determined from meridian obfervations; the azimuth of the firft ftar, and the altitude of the laft, at the time of their refpective paffages, may be determined by computation; which will give the altitude and azimuth of the heavenly body, for the time of the middle obfervation, when it paffed the interfection of the two wires.

The fame end may alfo be obtained by taking the obfervations in an inverted order; that is, by chufing a ftar at fuch an altitude, that the heavenly body fhall in a competent time afterwards arrive at the fame altitude, &c. but, as in thefe latitudes the alteration of azimuth is, efpecially in thofe parts that are in the neighbourhood of the zodiack, quicker than that of altitude, I apprehend it to be eafier to follow the flower motion with the fcrew, fo as to preferve the interfection, than the quicker, and therefore in general to be preferred; but where it happens otherwife, or the ftars lay more conveniently, the inverfe method may be purfued.

It is true, that fome degree of dexterity and practice may be requifite in the obferver in managing

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the ſet ſcrew, ſo as to keep the object interſected by the wire; but if fine ſmooth ſcrews, ſuch as are uſed for micrometers to aſtronomical quadrants, are adapted to the inſtrument, as well that commanding the horizontal motion as the vertical, I apprehend, the management will be perfectly eaſy and familiar to an obſerver otherwiſe well practiſed.

It is eaſy to ſee, that thoſe ſtars are to be preferred that are neareſt the heavenly body to be obſerved; and that, *cæteris paribus*, thoſe in ſuch poſitions, as riſe or fall ſlow, are beſt for determining their altitude; and thoſe that alter their azimuth ſlow, are beſt for determining the azimuth.

To avoid intricacy in deſcription, I have ſuppoſed only two wires interſecting each other at a right angle, in the focus of the teleſcope: but, for the ſake of getting a medium in ſuch parts of the obſervations as depend on time, it will be proper to have, not only three perpendicular wires, parallel to each other as common, but alſo three horizontal wires; the proportional diſtances of which being previouſly determined by obſervation, the oblique motions may (in parts not near the pole) be conſidered as right lines.

This method is the more valuable as it is entirely free from the knowledge of refractions; for ſince the computation gives the real altitude from the time given independent of refractions; and ſince the heavenly body is equally affected by refraction, at the ſame altitude; the computed altitude of the ſtar will give the real altitude of the heavenly body cleared of refraction, which never enters the queſtion: and ſince ſuch ſtars may be choſen as will render the time in-

tercepted

tercepted ſhort, there is the leſs chance of a change of refraction, during the time, between the middle and laſt obſervation; and therefore this method will be particularly uſeful in obſervations near the horizon.

Auſthorpe, April 17,
1768.

J. Smeaton.

N. B. Obſervations of this kind may be made upon the planets in the day light, by making uſe of the Sun for the firſt obſervation, inſtead of a ſtar; and waiting afterwards for the appearance of the ſtars.

XXVI. *A Specimen of a new Method of comparing Curvilineal Areas; by which many ſuch Areas may be compared as have not yet appeared to be comparable by any other Method.* By John Landen, *F. R. S.*

Read June 9, 1768.

WHEN a body in motion is continually acted upon by a variable force, the ſpace it has paſſed over at the end of any given time, it is well known, will be expreſſed by the area of the curve, whoſe ordinate expreſſes the velocity of the body, whilſt the time it has been in motion is expreſſed by the correſponding abſciſſa. Therefore the facilitating the computation of curvilineal areas will manifeſtly contribute to the improvement of the doctrine of motion. Which doctrine being a branch of philoſophy of no ſmall importance, ſuch improvement will not, I am perſuaded, be looked upon as a trifling ſpeculation, by the Royal Society: to whom, therefore, I do myſelf the honour of communicating this ſpecimen of a new and ready method of computing ſuch areas, by means of a given area; preſuming that what is here written will not be deemed undeſerving of a place in the Philoſophical Tranſactions.

I.

Geometricians have found, that, if A be put to denote the *whole* area of the curve, whoſe abſciſſa is x, and ordinate $\overline{1-x}^{\,p-1} \times x^{2r-1}$, the

the *whole* area of the curve, whose abscissa is x, and ordinate $\overline{1-x^2}|^{p-1} \times x^{2n+2r-1}$ will be $= \frac{r.r+1.r+2\ (n)}{p+r.p+r+1.p+r+2\ (n)} \times A$; n being any positive integer, and p and r any positive numbers, whole or fractional.

II.

By the preceding article, the *whole* area, when the ordinate is $\overline{1+x}|^{p-1} \times x^{2r+2z-1}$ is $= \frac{z.z+1\ (r)}{p+z.p+z+1\ (r)} \times \frac{1.2\ (z-1)}{p.p+1\ (z)} \times \frac{1}{2}$; the *whole* area, when the ordinate is $\overline{1-x^2}|^{p-1} \times x^{2z-1}$, being $= \frac{1.2\ (z-1)}{p.p+1\ (z)} \times \frac{1}{2}$.

Likewise, by the same article, the same *whole* area is $= \frac{r.r+1.r+2\ (z)}{p+r.p+r+1.p+r+2\ (z)} \times A$. Therefore this last expression is $= \frac{z.z+1\ (r)}{p+z.p+z+1\ (r)} \times \frac{1.2\ (z-1)}{p.p+1\ (z)} \times \frac{1}{2}$. From which equation, p and r being positive, as before observed, A, the *whole* area of the curve, whose ordinate is $\overline{1-x^2}|^{p-1} \times x^{2r-1}$, is found equal to $\frac{1.2.3\ (r+z-1) \times p+r.p+r+1\ (z)}{p.p+1.p+2\ (r+z) \times r.r+1\ (z)} \times \frac{1}{2}$, being any number whatever.

Consequently, supposing z infinite, we find A = the ultimate value, or limit of $\frac{1.2.3\ (z) \times p+r.p+r+1\ (z)}{p.p+1.p+2\ (z) \times r.r+1\ (z)} \times \frac{1}{2z}$.

Having thus obtained a general expression for the *whole* area of any curve, whose ordinate is expressed by $\overline{1-x^2}|^{p-1} \times x^{2r-1}$, and that expression

expreſſion for ſuch area conſiſting of an infinite number of factors multiplied together; to render the ſame uſeful in practice, ſome theorems are requiſite for aſcertaining the limits of ſuch products. The theorems which I have hitherto been able to inveſtigate ſuitable tohat purpoſe, I ſhall give in the next two articles.

III.

The limit of $\overline{1-m^2} \times \overline{2^2-m^2} \times \overline{3^2-m^2}\ (z) \times \frac{N^{2z}}{z^{2z+1}}$ is $= \frac{2}{m} \times$ ſine of mS; N being the number whoſe hyp. log. is 1, and S the ſemi-periphery of the circle, whoſe radius is 1.

Whencc, by taking m equal o, we find the limit of $1^2 . 2^2 . 3^2\ (z) \times \frac{N^{2z}}{z^{2z+1}} = 2S$,

IV.

The limit of $\overline{dz+a} \times \overline{dz+a+d} \times \overline{dz+a+2d} \times \frac{N^z}{2^{2z} d^z z^z}$ is $= 2^{\frac{a}{d}-\frac{1}{2}}$.

Hence, $\overline{z+1} . \overline{z+2} . \overline{z+3}\ (z)$ being $= 1 . 3,5\ (z) \times 2^z$, it appears, that the limit of $1 . 3 . 5\ (z) \times \frac{N^z}{2^z z^z}$ is $= 2^{\frac{1}{2}}$.

I ſhall now give ſome examples, to ſhew the uſe of the above

Writing

V.

Writing A, B, C, D, and E, for the *whole* areas of the curves, whose ordinates are $\frac{x^{\frac{2}{3}}}{\overline{1-x^2}|^{\frac{1}{2}}}$, $\frac{x^{\frac{1}{3}}}{\overline{1-x^2}|^{\frac{1}{2}}}$, $\frac{1}{\overline{1-x^2}|^{\frac{1}{2}}}$, $\frac{x^{-\frac{1}{3}}}{\overline{1-x^2}|^{\frac{1}{2}}}$, and $\frac{x^{-\frac{2}{3}}}{\overline{1-x^2}|^{\frac{1}{2}}}$, respectively; we have, by Art. II.

$$A = \text{the limit of } \frac{1.2.3\ (z) \times 4.\ 7.10\ (z)}{1.3.5\ (z) \times 5.11.17\ (z)} \times \frac{2^{2z-1}}{z};$$

$$B = \text{the limit of } \frac{1.2.3\ (z) \times 7.13.19\ (z)}{1.3.5\ (z) \times 2.\ 5.\ 8\ (z)} \times \frac{1}{2z};$$

$$C = \text{the limit of } \frac{1^2.\ 2^2.\ 3^2\ (z)}{1^2.\ 3^2.\ 5^2\ (z)} \times \frac{2^{2z-1}}{z} = \left\{\begin{array}{l}\text{the area of the semi-circle,} \\ \quad\text{whose radius is } 1;\end{array}\right.$$

$$D = \text{the limit of } \frac{1.2.3\ (z) \times 5.11.17\ (z)}{1.3.5\ (z) \times 1.\ 4.\ 7\ (z)} \times \frac{1}{2z};$$

$$E = \text{the limit of } \frac{1.2.3\ (z) \times 2.\ 5.\ 8\ (z)}{1.3.5\ (z) \times 1.\ 7.13\ (z)} \times \frac{2^{2z-1}}{z}.$$

Now it appears by the above equations, that $\frac{A}{B}$ is = the limit of $\frac{2.4.\ 5.\ 7.\ 8.10\ (2z)}{5.7.11.13.17.19\ (2z)} \times 2^{2z}$; which by Art. III. is $= \frac{6 \times \text{sine } 60°}{12 \times \text{sine } 30°} = \frac{3^{\frac{1}{2}}}{2}$.

Therefore A is $= \frac{3^{\frac{1}{2}} B}{2}$.

 It

It appears also, that $\frac{B \times D}{C}$ is $=$ the limit of $\frac{5 \cdot 7 \cdot 11 \cdot 13 \cdot 17 \cdot 19\ (2z)}{2 \cdot 4 \cdot 5 \cdot 7 \cdot 8 \cdot 10\ (2z)} \times \frac{3}{2^{2z}+1}$; which by Art. III. is $= 3^{\frac{1}{2}}$.

$$\text{Therefore D is} = \frac{3^{\frac{1}{2}}C}{B}.$$

It likewise appears, that

$$B \times E \text{ is} = \text{the limit of } \frac{1^2 \cdot 2^2 \cdot 3^2\ (z)}{1^2 \cdot 3^2 \cdot 5^2\ (z)} \times \frac{3 \cdot 2^{2z-1}}{z} = 3\,C.$$

$$\text{Therefore E} = \frac{3\,C}{B}.$$

VI.

Writing F, G, H, I, and K, for the *whole* areas of the curves, whose ordinates are $\frac{x^{\frac{2}{3}}}{\overline{1-x}|^{\frac{1}{3}}}$, $\frac{x^{\frac{1}{3}}}{\overline{1-x}|^{\frac{1}{3}}}$, $\frac{1}{\overline{1-x}|^{\frac{1}{3}}}$, $\frac{x^{-\frac{1}{3}}}{\overline{1-x}|^{\frac{1}{3}}}$, and $\frac{x^{-\frac{2}{3}}}{\overline{1-x}|^{\frac{1}{3}}}$, respectively, we have, by Art. II.

$$F = \text{the limit of } \frac{1 \cdot 2 \cdot 3\ (z) \times 9 \cdot 15 \cdot 21\ (z)}{2 \cdot 5 \cdot 8\ (z) \times 5 \cdot 11 \cdot 17\ (z)} \times \frac{3^z}{2z};$$

$$G = \text{the limit of } \frac{1 \cdot 2 \cdot 3\ (z) \times 8 \cdot 14 \cdot 20\ (z)}{2^2 \cdot 5^2 \cdot 8^2\ (z)} \times \frac{3^z}{2^{z+1} \times z};$$

$$H = \text{the limit of } \frac{1 \cdot 2 \cdot 3\ (z) \times 7 \cdot 13 \cdot 19\ (z)}{2 \cdot 5 \cdot 8\ (z) \times 1 \cdot 3 \cdot 5\ (z)} \times \frac{1}{2z} = B;$$

I = the

$$I = \text{the limit of } \frac{1^z.\,2^z.\,3^z\,(z)}{2.5.8\,(z)\times 1.4.7\,(z)}\times\frac{3^{2z}}{2z},$$

$$K = \text{the limit of } \frac{1.2.3\,(z)\times 5.11.17\,(z)}{2.5.8\,(z)\times 1.\ 7.13\,(z)}\times\frac{3^z}{2z}.$$

By which equations it appears, that $\frac{F}{H} = \frac{F}{B}$ is = the limit of $\frac{1.3.\ 3.\ 5.\ 5.\ 7\,(2z)}{5.7.11.13.17.19\,(2z)}\times 3^{2z}$; which by Art. III. is $= \frac{4\times\text{fine of }90^\circ}{12\times\text{fine of }30^\circ}$ $\frac{2}{3}$.

Therefore F is $= \frac{2B}{3}$.

It appears likewife, that

$$\frac{G}{B}\text{ is } = \text{the limit of } \frac{1.3.5\,(z)\times 8.14.20\,(z)}{2.5.8\,(z)\times 7.13.19\,(z)}\times\frac{3^z}{2^z}$$

$$= \text{the limit of } \frac{1.3.5\,(z)\times 4.7.10\,(z)}{4.7.10\,(2z)}\times 6^z$$

$$= \text{the limit of } \frac{1.3.5\,(z)}{3z+4.3z+7.3z+10\,(z)}\times 6^z;$$

which, by Art. IV. is $= 2^{-\frac{1}{3}}$.

Therefore G is $= \frac{B}{\frac{1}{4}}$.

It alfo appears, that

I is = the limit of $\frac{1^z.\,2^z.\,3^z\ (z)}{2.3.5.7.8.10\ (2z)}\times\frac{3^{2z+1}}{2}$; which, by Art. III. $= \frac{2S}{4\times\text{fine of }60^\circ} = \frac{S}{3^{\frac{1}{2}}} = \frac{2C}{3^{\frac{1}{2}}}$.

Moreover

Moreover it appears, that

$$\frac{B \times K}{I} \text{ is } = \text{the limit of } \frac{1 \cdot 4 \cdot 7 \ (z) \times 5 \cdot 11 \cdot 17 \ (z)}{2 \cdot 5 \cdot 8 \ (z) \times 1 \cdot 3 \cdot 5 \ (z)} \times$$

$$3^z$$

$$\text{the limit of } \frac{3z+2 \cdot 3z+5 \cdot 3z+8 \ (z)}{1 \cdot 3 \cdot 5 \ (z)} \times \frac{1}{2^z \cdot 2^{z-1}} \text{; which,}$$

by Art. IV. is $\frac{3}{2^{\frac{1}{3}}}$.

Therefore K is $\frac{3\,1}{2^{\frac{1}{3}} B} = \frac{2^{\frac{2}{3}} \cdot 3^{\frac{1}{2}} C}{B}$

And, in like manner, may a great number of other areas be compared.

Note. All the *whole* areas above-mentioned are ſuppoſed to begin where x begins, and to ſtand upon a baſe = 1.

Received

Received December 12, 1765.

*XXVII. *Experiments and Obſervations upon a blue Subſtance, found in a Peat-moſs in* Scotland: *By* Sylveſter Douglas, *Eſquire.*

Read February 13, 1766.

THE blue coloured ſubſtance, which is the ſubject of the following obſervations, and which is now before you, was accidentally dug up in the ſummer 1759, in order to mix with ſome other materials for the purpoſe of manure, to be laid on ſome ground, at preſent in my poſſeſſion, in the north of Scotland, about twelve miles from Aberdeen.

I have not met with a deſcription of this ſubſtance in any naturaliſt. Kentman indeed in a few lines mentions a blue earth, which he calls *cœruleum Patavinum*, which agrees with the ſubſtance I am about to deſcribe, in one remarkable circumſtance; that it is at firſt of a white colour, and becomes blue only in conſequence of being expoſed to the air. Mr. Da Coſta's *ochra friabilis cœrulea* †, would alſo have been found probably to correſpond with it, if a more particular account could have been given of the circumſtances in which it was found, and its appearance before the air had acted upon it. Mr. Cronſtedt, in his

* This paper, having being miſlaid, could not be printed in the Volume of the Philoſophical Tranſactions for 1766.

† Nat. Hiſt. of Foſſ. p. 103.

his late Syſtem of mineralogy, mentions a blue ſubſtance, which ſeems to be of the ſame kind, and which, I think, he ſays, is found ſomewhere in Pruſſia. His account of it is very ſhort; and I am not very certain with regard to it, as I have not the book by me.

The place, where it is dug up, is of a marſhy nature, in the corner of an exhauſted peat-moſs. Immediately under the ſward lies a *ſtratum*, about a foot deep, of common peat; next to that is the ſubſtance itſelf, with irregular *ſtriæ* of a peaty matter all through it, to the depth of near another foot; and below this, I think, there is clay. While it is thus wet, and ſhut out from the air, it is of a white colour, and ſeemingly of a fatty conſiſtence, not unlike lime that has been prepared for cement. All the water in the neighbourhood of the place is in ſome meaſure impregnated with iron. When this ſubſtance is expoſed to the air, it gradually as it dries aſſumes the blue colour; the peaty matter intermixed with it continuing of the ſame appearance as before. The whole mixed maſs is of a very friable texture, eaſily crumbling betwixt the fingers; and the blue part, gently rubbed between them, feels like a fine impalpable powder. It has hardly any ſenſible taſte; what it has, approaches a little to that of ſulphur: the ſmell, when it is firſt taken up, is ſenſibly ſulphureous, and if a piece of paper, with part of it adhering to it, be kindled, it ſhews a flame ſimilar to burning ſulphur.

The only means of ſeparating it from the black matter is by elutriation. When water is poured on it, and they are ſhaken together, and then left at reſt for ſome time, the black part ſubſides to the bottom,

and the blue can be poured off ſtill diffuſed in the water, from which however it ſoon ſeparates, and falls to the bottom. It is not poſſible entirely to free the blue from the peaty matter, for, after above twenty different additions of water, there were ſtill ſtreaks of black interſperſed through it, when it was allowed to ſubſide; neither have I ever been able to ſeparate all the blue from any of the black part.

When a little water is added to a quantity of it, it acquires ſome degree of tenacity, and when a ſmall portion of water is allowed to ſtand on its ſurface for a day or two, the water becomes of a yellowiſh colour.

Theſe are the chief circumſtances relating to its natural hiſtory, and the obvious properties it diſcovers without the aſſiſtance of chemical operations. The following are the experiments I have made upon it, with a view to diſcover its nature more particularly.

In order to find whether there was any part of it ſoluble in water, I paſſed a large quantity of water, which I had uſed in ſeparating the black from it, through a filter, and then ſet it to evaporate in B M; but there was nothing left in the veſſel after the evaporation, except ſome earth, which the water had probably contained in itſelf.

To a quantity of the blue powder, I added the common vitriolic acid of the ſhops; a degree of efferveſcence enſued, and a conſiderable froth remained for ſome time on the ſurface; the whole was changed into a dark brown colour, and, when filtrated, the ſolution was a tranſparent brown liquor. A conſiderable ſediment remained behind on the filter; but I am inclined to think, that this conſiſted chiefly of the peaty

peaty matter, which had not been entirely ſeparated; for when the experiment was repeated ſeveral times with different parcels of the blue, it appeared more or leſs ſoluble according as the black had been more or leſs perfectly ſeparated; and when I added the vitriolic acid to a quantity of the black, though it turned it all of a brown colour, it only ſeemed to diſſolve a quantity equal to the portion of blue, which ſtill adhered to it.

The nitrous acid, added to the blue powder, produced pretty much the ſame effects, only the filtrated ſolution was of a much lighter brown.

The fixed vegetable alkali diſſolved alſo a conſiderable part of it; but whether the whole or not, I cannot ſay. The ſolution was an opaque brown liquor, which did not become tranſparent after being twice filtrated, though it depoſited no ſediment upon ſtanding ſeveral days.

I added a ſmall quantity of volatile alkali to it, which ſeemed to diſſolve part of it, and turned the reſt obſcurely green.

To the ſolution in vitriolic acid, I joined ſome fixed vegetable alkali: an efferveſcence aroſe, and a light curd of a colour between green and blue was thrown to the top, which ſoon ſubſided, and became white.

A ſimilar præcipitate was obtained from the nitrous acid, only it was not at firſt thrown up to the ſurface in the ſame manner as the foregoing.

From the ſolution in fixed vegetable alkali, a reddiſh brown præcipitate was obtained, by the addition of vitriolic acid. Equal quantities of the blue powder and of black flux were mixed together, and being

put into a crucible, were kept in a ſtrong degree of heat for ſeveral hours: on being removed, and taken out of the crucible, the whole was found concreted into a ſpungy maſs, the bottom of which was cruſted over with ſomething that had a kind of metallic appearance. This maſs was powdered, and the lighter parts waſhed off; after which, a magnet was applied to what remained, and it attracted many of its particles ſtrongly, without being brought in contact with them.

Part of the white præcipitate from vitriolic acid was mixed with a little fixed alkali, and being laid on a piece of charcoal, the flame of a candle was directed to it by means of a blow-pipe. It was thus kept in a red heat for about an hour; and on being removed, the magnet was applied to it, but none of the powder was attracted by it. The quantity that can be examined in this way does not exceed a few grains.

To a ſmall quantity of the white præcipitate, I added an infuſion of tea; which turned it blue, approaching to the original colour, but not ſo deep.

To another parcel of the ſame, I added ſome infuſion of galls, and ſhook them together. The liquor became of a dark blue colour, and what part of the powder remained at the bottom of the glaſs was of the ſame colour. This was not ſo bright as that of the original powder diffuſed in water, but entirely ſuch as might be expected from the diffuſion of it in a brown liquor like infuſion of galls; and, to ſhew this, I poured ſome of the infuſion of that aſtringent on the blue ſubſtance itſelf, and on ſhaking them together, they produced a colour almoſt entirely the ſame.

 A quan-

A quantity of the brown ſolution in vitriolic acid was diluted with water till it became very pale. I then poured to it ſome infuſion of galls, which turned it immediately black.

A parcel of the blue ſubſtance, being placed at the diſtance of a foot from the fire, was changed to a greeniſh colour.

Theſe experiments, compared with its natural hiſtory, ſeem to throw ſome light on the nature and compoſition of this curious production. It is the known property of all vegetable aſtringents, to affect the colour of iron, either when it is combined with vitriolic acid, in the form of green vitriol, or by itſelf; and I believe they have no ſuch effect on any other metal. The colour they produce with it is various, inclining indeed to black, but almoſt of every different ſhade between black and blue; and it ſeems to me, that they occaſion a more pure black with vitriol, and a purple blue with iron itſelf, as is ſeen for inſtance on dropping a little infuſion of tea on a knife.

Now we find, that when a vegetable aſtringent is added to a ſolution of this ſubſtance in vitriolic acid, it ſtrikes a black colour with it, and reſtores the original blue to the white præcipitate from that acid. We alſo find, that there actually is iron contained in it; becauſe, when fluxed with the black flux, its particles are attracted by the loadſtone; and we can draw no argument, from our not having diſcovered iron in the experiment with the blow-pipe, againſt the preſence of that metal, as ſo little can be examined in that way. I therefore think it probable, that the principal ingredients of it, and thoſe on which its colour

colour depends, are iron and ſome vegetable aſtringent. The ſituation in which it is found favours this conjecture very ſtrongly; for, in the firſt place, the water in the neighbourhood of it is all impregnated with iron; and ſecondly, in almoſt every peat-moſs, there are the remains of oak trees, ſtill freſh diſperſed through them; and both their wood and bark are of a ſtrong aſtringent nature.

I do not pretend to ſay, that theſe are its only ingredients. I think we may conclude, from the lightneſs of the ſubſtance, that iron does not form a very great part of it; and the ſmell, and the particular flame it exhibits in burning, would ſeem to ſhew the preſence of ſulphur in it. This, however, can be only in a very ſmall proportion, ſince ſo much of it is ſoluble in acids, which do not at all affect ſulphur. I ſuppoſe the præcipitate from acids conſiſts chiefly of iron and earth.

I have made ſome trials on the blue powder after it was partly well freed from the black matter, in order to ſee how far it might be uſeful as a paint: a quantity of it was rubbed in a glaſs mortar with oil of walnuts; but, after being thoroughly mixed with the oil, its colour was changed to black. It is probable, therefore, that little can be expected from it as an oil colour; but it retains its natural brightneſs when mixed with gum-water; and, as it is naturally in a very fine powder, it is diffuſed intimately through it without any difficulty, ſo that, if it could be got in ſufficient quantity, it would be a cheap and uſefull water-colour. I think there is reaſon to believe, that it might be found in moſt peat-moſſes, as what ſeem to be the materials of which it is compoſed are preſent

ſent almoſt in all of them. Two or three years ago a gentleman ſent me a parcel of it, which he found in a moſs on his eſtate, five or ſix miles diſtant from the place where I firſt obſerved it. I am informed, that Mr. Da Coſta has had ſpecimens of a blue earth ſent him from different parts of England: what Sir Hans Sloane gave him from Ireland ſeems alſo to have been the ſame, and, from what I have quoted from Kentman and Cronſtedt, it would appear, that it is obtained on ſeveral parts of the continent. From all this, I think we may conclude, that it might be procured in ſufficient quantity to be a cheap paint; particularly as it is in a manner levigated and prepared by nature.

It is to be lamented, that its colour is ſo eaſily affected by alkalies, eſpecially the volatile alkali, which abounds ſo much in the atmoſphere in towns, and by any conſiderable degree of heat. I have, never however, found any change produced on it from being expoſed for a conſiderable time to the air, or to the heat of a room where a fire was kept conſtantly burning.

XXVIII. *Two Medical Obſervations by Dr.* Joſeph Benevuti, *Phyſician at* Lucca; *communicated to the late Preſident of the Royal Society, by Dr.* Ch. Allioni *of* Turin, *F. R. S. and tranſlated from the* Latin *by* Daniel Peter Layard, *M. D. Phyſician to her Royal Highneſs the Princeſs Dowager of* Wales, *Member of the Royal College of Phyſicians in* London, *and of the Royal Societies of* London *and* Gottingen.

I. *Of a ſick Man ſurprizingly recovered from a Fever.*

Read June 9, 1768.

A MAN forty years of age, named Angelus Amadei, of a plethoric conſtitution, and of a low ſize, having a malignant fever, began on the ninth day to grow delirious, and continued ſo during the tenth night; when, ſeveral bad ſymptoms appearing, it was thought he muſt die ſoon. Early on the eleventh day in the morning, he bid the by-ſtanders quit his room, and expreſſed a deſire of going to ſleep; his friends were unwilling to withdraw, unleſs they firſt ſtripped him of his ſhirt, and dried him of the ſweat he was in. But the patient refuſing, and at laſt growing angry, they were obliged to yield to his will. About an hour after, a woman went into the bed-room, and not finding

finding the man, ſhe called the ſervants, who ſearched the houſe, and the well, into which they feared he had thrown himſelf; but to no purpoſe. In the mean time a rumour ſpread, as is uſual in ſuch caſes, that this had happened, either by the interpoſition of the devil, or by a miracle.

The keeper of the baths at Lucca gave orders for every body to make a diligent ſearch; and on the third day the ſick man was at laſt found in a vineyard, about two miles from his houſe, hidden in a hut, where, he ſaid, that the day before, he with great aſtoniſhment found himſelf, without at all knowing how he came there. It ſeemed to me, that he muſt have got down by the window of the bed-chamber, which was not far from the ground. What ſeems moſt extraordinary is, that, in order to quench his thirſt, this man ſwallowed a large quantity of ſnow (with which the earth was covered, it being in the winter); and that neither this ſort of drink, nor the cold air, did in the leaſt affect him; for though he had gone away from home all in a ſweat, and with no other covering than his ſhirt, yet he was freed from his fever, and is now reſtored to his former health.

II. *Of an extraordinary great Head.*

Not long ſince, I went to Benabii, a town ſituated in the territory of Lucca, to ſee a man, whoſe head, I had heard, was much larger than is uſual. The ſame curioſity procured me the honour of attending at the ſame place on Princeſs Lambertini, niece to Pope Benedict the XIVth, whoſe health I had the care of, while ſhe drank the water of our baths.

I ſaw

I ſaw a man, thirty years of age, and yet of the ſize of a boy ſeven years old, who was ſitting on a couch ſeat, with his head (which indeed was quite out of ſize) inclined on the right ſide, and reſting on a pillow; which, when he wanted to move, he ſupported with his hands, as it lay on a very ſmall neck. This man had enjoyed a good health till he was ſix years old; he then had a diarrhœa, which laſted nine months, and, upon its ſtopping, his lower extremities were ſeized with the palſy, and loſt their motion, but their feeling remained. From that time his head increaſed yearly, together with his face, noſe, ears, eyes, mouth, &c. but the remainder of his body did not grow at all. The circumference of his ſcalp meaſured thirty ſeven inches, and eight lines, Engliſh meaſure. The length of his face was twelve inches and three lines. Theſe meaſures were taken by the ſaid princeſs and ſeveral of her attendants. This man eats greedily, ſleeps well, but diſcharges his *fæces* and his urine involuntarily. The ſtrength which he has in his hands is very ſurprizing, being ſuch, that it is difficult for any perſon to get looſe from him, when once he holds faſt. He is beſides quick as to his underſtanding, he talks, and has a good memory; ſeldom or never forgetting what he may have read in books.

XXIX. *An Account of a particular Species of Cameleon: By* James Parſons, *M. D. F. R. S.*

Read June 12, 1768.

AMONG the quadrupeds of the earth, the claſs of Cameleons is one of the moſt curious families; inſomuch as to have engaged the attention of many natural hiſtorians; not only on account of the particular ſtructure of its parts, but alſo of ſeveral curious phænomena which are peculiar to it, in its ſeveral ſpecies, in the different parts of the world.

This animal is ranged by authors under the generical name *Lacerta*, which comprehends a great variety of all ſizes from the Crocodile to the ſmalleſt Lizard: but as the Cameleon has its various ſpecies, and each ſuch properties as are not common to any others under the tribe of *Lacertæ*, they indeed deſerve to be regarded as a particular *genus*.

However, ſince authors have been very full in their accounts of theſe creatures; which every one, curious in their enquiries into the hiſtory of animals, may have recourſe to, collected in an excellent work intitled, *Dictionaire raiſonné des Animaux*, I ſhall only entertain the learned Society with a deſcription of a ſpecies of Cameleon which I conſider as a non-deſcript, having made a careful reſearch concerning this animal among authors, and ſeen ſeveral kinds of them, as well as various figures in every hiſtory I am acquainted with; from all which the ſubject before us is very different.

It

It is chiefly in the ſtructure of the head that this difference appears, and its ſingularity induced me to obſerve it with attention; for the head is very large in proportion to the reſt of this animal, and all others of the ſame claſs; and the more ſo, if we meaſure from the two anterior flat proceſſes, to the poſterior extremity or proceſs of the *cranium*, which meaſures three inches and a quarter. This poſterior proceſs extends backwards, over the neck, to the firſt vertical proceſs of the ſpine; and the interior proceſſes, one on each ſide, project forwards and upwards in an oblique direction over the naſal hole, and are bluntly ſerrated all round; the ſurface of the entire face is covered with tubercles and ſcales, which, by being in a dry ſtate, have loſt their protuberance and luſtre, which the ſcales certainly were endowed with while the animal was alive.

The length of the two mandibles is equal, and is two inches and a quarter from the articulation of the lower with the upper jaw, to the apex of each; both being furniſhed with a fine ſet of ſmall pointed teeth; all of a ſize, and ſo ſet, that, upon the animal's cloſing his mouth, the teeth do not meet, but thoſe of the upper fall in with thoſe of the under alternately. There are no molares nor canine teeth.

The orbits are extremely large and deep, ſo that this Cameleon muſt have had very great eyes, and very globular; for they are each more than a third of the whole length of the mandible in diameter.

From a cloſe inſpection of the ſkin, which is now contracted and dried cloſe to the ſkeleton, it appears ſcaled all over; the larger ſcales are upon part of the head and upon the ſides of the neck; the ſmaller, under the jaws, upon the neck, and over the whole

 body

body; but we can form no idea of its proper colour whilſt the animal is alive, yet do not doubt of its having had a very beautiful covering.

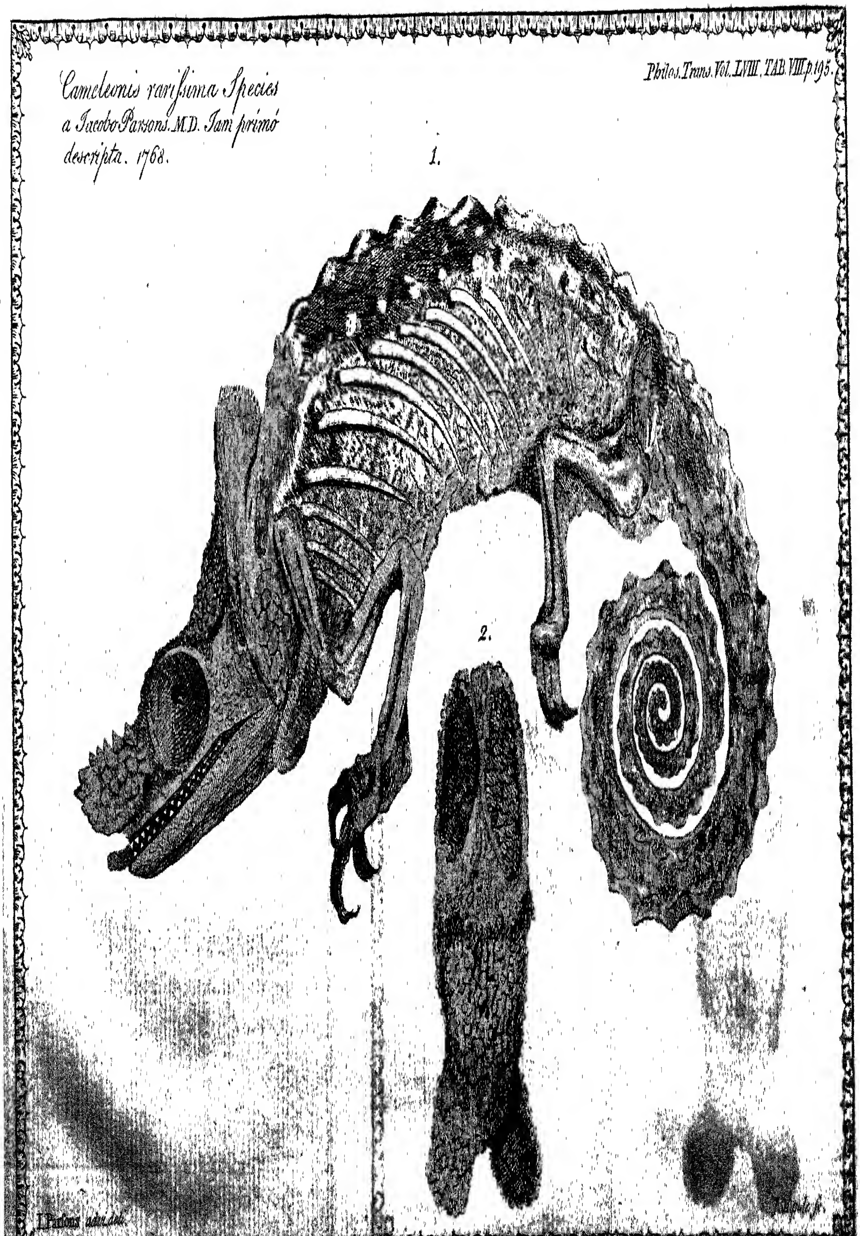
Almoſt every ſpecies of *Lacerta* have five fingers upon each extremity; all the Cameleons have them, but they differ in the diſpoſition of the fingers; this before us has the tarſal, metatarſal, and three bones to each finger, as it is in human hands: in this Cameleon the fingers are very long, and terminated with pointed nails bending downwards; three of the fingers of each anterior extremity are inwards in the place of the thumb, and the other two are outwards; whereas in the poſterior extremities, three are outwards, and the other two inwards, having between them ſuch a large ſpace, or diviſion, as is between the thumb and fingers of men.

But this diſtribution of the fingers I ſaw in one of the triangular-headed Cameleons: other ſpecies have the five fingers together, and very ſhort like ſtumps; but that deſcribed by Pitfield from the diſſections of the Royal academy, has its fingers diſpoſed in the ſame manner with this, and is one of thoſe with a triangular head and creſt.

The vertical edge of the ſpine is ſcolloped all along from the neck to the extremity of the tail, and has on each ſide a row of knobs, or proceſſes, as far as the articulation of the thigh with the bone that runs up towards the ſpine; but from thence, where the tail begins, there is a ſecond lateral row of knobs, which continue all along the tail.

There does not appear any paſſage into the head for hearing, nor any other but the mouth and naſal holes; which is alſo taken notice of by the Royal

Academy


Philos. Trans. Vol. LVIII. TAB. VIII. p. 195.
Cameleonis rarissima Species a Jacobo Parsons. M.D. Jam primó descripta. 1768.
1.
2.

Academy in their obſervations upon that mentioned above. This made Bellonius imagine, that theſe naſal holes ſerve Cameleons for hearing as well as breathing; ſo that it ſhould ſeem, that more ſpecies than one are deſtitute of auditory holes.

This ſubject came into my hands from the owner Mr. Millan, who was kind enough to leave it with me for the purpoſe of laying it before the Royal Society; we have no knowledge of its native place, as he bought it among other natural productions now in his collection.

T A B. VIII.

The firſt figure repreſents the animal in profile.
The ſecond is a view of the face, or upper ſurface of the head.

XXX. *A Letter from* J. A. Rizzi Zannoni, *Member of the Academy of Sciences at* Gottingen, *and Geographer to his* Sicilian *Majesty, to the late Earl of* Morton, *Pr. R. S. containing several Astronomical Observations, made in several Parts of the Kingdom of* Naples *and* Sicily; *translated from the* French, *by* Mathew Maty, *M. D. Sec. R. S.*

Naples, July 29, 1768.

My Lord,

Read Nov. 10, 1768.

I Take the liberty to apply to your Lordship on a subject of great importance to geography. His Sicilian Majesty has lately ordered a topographical map of his dominions to be made; and all the materials, which are to serve for that work, have been collected together.

We frankly acknowledge, my Lord, that we are indebted to Englishmen for most of the astronomical observations, made in various parts of the kingdom of Naples. The following list contains them all; but, in order to render them useful, it would be necessary to have the corresponding ones made at London, or Greenwich, the latitude and longitud of these two places being perfectly known, by the accurate observations of the mathematicians bellonging to the Royal Society.

We,

We, therefore, beg of your Lordſhip to procure us, from the members of that illuſtrious body, the obſervations relative to our object. As for the computation of the parallaxes, and the reſults, I will ſet about that work myſelf, as ſoon as I ſhall be furniſhed with the proper materials.

I have the honor to be,

My Lord,

Your Lordſhip's

moſt obedient humble ſervant,

J. A. Rizzi Zannoni.

Eclipses of the three first Satellites of Jupiter, observed at the Royal College at Naples, in the years 1762, 1763, and 1764, by Father Maria Carcani, Superior of the Pious Schools, by means of a telescope of 24 palms *.

Year	Month	Day	h	m	s	Observation
1762	August	4 at	1	35	9	Immerf. of I. Satellite.
	Sept.	26 —	4	2	17	Immerf. of I. Satellite.
	Octob.	19 —	4	19	44	Immerf. of I. Satellite.
	Nov.	14 —	7	43	19	Emerf. of I.
		—		or	20	Emerf. of I.
	———	28 —	11	24	52	Emerf. of I.
	———	30 —	5	52	14	Emerf. of I.
	———	30 —	10	45	49	Emerf. of II.
	Decem.	14 —	9	36	33	Emerf. of I.
	———	25 —	7	47	56	Emerf. of II.
1763	Jan.	6 —	9	39	36	Emerf. of I.
	———	22 —	7	54	7	Emerf. of I.
	———	29 —	9	47	35	Emerf. of I.
	March	9 —	8	26	49	Emerf. of I.
	———	9 —	8	11	9	Emerf. of III.
	———	25 —	6	55	24	Emerf. of I.
	Octob.	12 —	15	54	25	Immerf. of II.
	———	14 —	17	55	45	Immerf. of I.
	———	30 —	10	27	31	Immerf. of II.
1764	Jan.	4 —	5	39	3	Emerf. of I.
	———	25 —	11	15	23	Emerf. of I.
	Feb.	10 —	9	33	0	Emerf. of I.
	———	17 —	11	29	0	Emerf. of I.
	———	19 —	5	58	47	Emerf. of I.
	———	26 —	7	55	30	Emerf. of I.
	March	4 —	9	51	25	Emerf. of I.
	———	13 —	6	18	8	Emerf. of I.
	———	27 —	10	14	51	Emerf. of I.

* The Italian palm is about two-thirds of a foot.

1760

Year	Month	Day	h	′	″	
1760	Aug.	11 at	10	17	20	Imm. of I. at Meſſina, by Mr. Robert Dunn.
	——	20 —	8	57	28	Emerſ. of I. at Meſſina, by Mr. Robert Dunn.
	——	27 —	10	52	47	
	Sept.	3 —	12	49	55	
	——	11 —	2	48	5	Emer. of I. at Reggio, by Mr. Dunn.
	——	12 —	9	17	49	
	Oct.	5 —	9	46	11	Emer. of I. at Cotrone, by Mr. Dunn.
	——	12 —	11	44	34	
	July	3 —	11	45	10	Imm. of I. at Malta, by Mr. Dunn.
1762	Sept.	11 —	12	16	38	Imm. of I. at Policaſtro, by Mr. Berkeley.
	——	19 —	2	12	36	
	Oct.	4 —	12	35	53	Imm. of I. at Cirella, by Mr. Berkeley, with a Newtonian Reflector of 3 feet, of Watkins.
	——	12 —	2	31	37	
	——	13 —	9	0	31	
	——	19 —	4	25	56	
	——	20 —	10	56	37	
1764	May	5 —	9	20	49	Em. of I. at Sancta Maura.
	Aug.	10 —	15	14	6	Imm. of I. at Linguetta.
	Sept.	2 —	15	29	51	Imm. of I. at Durazzo, by Mr. Berkeley.
	——	3 —	15	54	50	
	——	18 —	13	48	6	
	——	25 —	15	48	0	
	May	28 —	9	21	32	Em. of I. at Tarentum.

The tranſit of Mercury over the Sun, in the year 1743, was obſerved at Naples, by Father P. N. with a teleſcope of 18 palms, made by Campani. He determined the firſt contact at $1^h\ 57'\ 25''$, and the ſecond at $2^h\ 0'\ 35''$.

The tranſit of the ſame planet in 1753, was obſerved at Naples, in the Royal College, by Father Carcani, with a teleſcope of $18\frac{1}{2}$ palms.

	h	′	″
Firſt contact at	23	5	51
Emerſ. of center	23	7	28
Second contact	23	9	5

The

The transit of Venus over the Sun, of the year 1761, was observed at Naples, at the same place, by the same astronomer, with an excellent telescope of 24 palms.

				″
The first contact	at	21	16	55
The second contact		21	35	20

The same transit was likewise observed at Malta, by several people. A serjeant in the marines, who is an excellent pilot, has posted himself at Valetta, and has an excellent clock, and a Newtonian reflecter of 3 palms. He observed the beginning of the emersion at $21^h\ 17'\ 50''$, and the total emersion at $21^h\ 36'\ 33''$.

Finally, at Tarentum, the latitude of which place is the same with that of Naples, Mr. William Felton observed the transit of Mercury over the Sun of 1753, with a very good reflector of 2 feet.

		h	′	″
The first contact	at	11	18	26
The second contact		11	21	36

Eclipses

Eclipſes of the three firſt Satellites of Jupiter, obſerved, at the Royal Obſervatory at Greenwich, in the years 1762, 1763, and 1764, during the time that the late Rev. Nathaniel Bliſs, A. M. F. R. S. Profeſſor of Geometry in the Univerſity of Oxford, was Aſtronomer Royal; communicated by Nevil Maſkelyne, B. D. F. R. S. Aſtronomer Royal.

N. B. The obſervations were ſometimes made with a 6 foot Newtonian reflector, the diameter of whoſe aperture is 9 inches, and which magnifies 100 times, and ſometimes with a 2 foot reflector, the diameter of whoſe aperture is $4\frac{1}{2}$ inches, and which magnifies 90 times; and ſometimes with another 2 foot reflector, made by Mr. Bird, whoſe aperture is 3,8 inches, which magnifies 88 times.

The 6 foot teleſcope ſhews an immerſion of the firſt ſatellite later, and an emerſion of the ſame ſooner, than the firſt of the 2 foot teleſcopes by about 20″. The difference between the two 2 foot teleſcopes may be ſuppoſed about 5″.

1762	Aug.	26 at	12	51	45		Imm. of I. ſatel. with 6 foot teleſcope.
	Sept.	18 —	13	9	29		
	Oct.	4 —	11	31	26		
	———	11 —	13	27	9		
	Nov.	3 —	15	48	56		Em. of I. with 6 foot teleſcope.
	———	21 —	15	44	41	a little hazy	
	Dec.	23 —	4	57	40		

	Jan.	6 at	8	42	6	Em. of I. ſatel.	with 6 foot teleſc.
	March	9	7	7	51	Em. of III. ſatel.	2 foot teleſcope, N. Maſkelyne.
			7	30	16	Em. of I. ſatel.	2 foot teleſcope, N. Maſkelyne.
1763	Sept.	30 —	13	7	52	Imm. of I. ſatel.	6 foot
	Oct.	12 —	14	58	5	Imm. of II. ſatel.	Bird's 2 foot
	——	14 —	16	59	1	Imm. of I. ſatel.	Bird's 2 foot
		16 —	11	28	0	Imm. of I. ſatel.	Bird's 2 foot
	Feb.	24 —	12	29	42	Em. of I. ſatel.	Bird's 2 foot
	March	4 —	8	54	48	Em. of I. ſatel.	Bird's 2 foot
	April	12 —	7	41	39	Em. of I. ſatel.	Bird's 2 foot
1764	Sept.	25 —	14	31	49	Imm. of I. ſatel.	2 foot
	Nov.	4 —	13	3	37	Imm. of I. ſatel.	2 foot
	Nov.	10	14	57	11	Imm. of I. ſatel.	6 foot
			14	56	57	Imm. of I. ſatel.	2 foot

The obſervations prior to the year 1762 fell in the time of Dr. Bradley's being Aſtronomer Royal, whoſe obſervations are not in my poſſeſſion; neither can I have acceſs to them.

Nevil Maſkelyne.

XXXI. *An*

Received October 4, 1768.

XXXI. *An Account of ſome Experiments, by Mr.* Miller *of* Cambridge, *on the ſowing of Wheat: By* W. Watſon, *M. D. F. R. S.*

To the Royal Society.

Gentlemen,

Lincoln's Inn Fields, Oct. 4, 1768.

Read Nov. 24, 1768.

HAVING been informed, that in the Botanic garden at Cambridge, there had been produced, by the ingenuity and care of Mr. Charles Miller, the gardener there, from one grain of wheat only, in little more than a year, a much more conſiderable quantity of grain, than was ever attempted, or even conjectured to be poſſible; I have deſired him to ſend me a particular account thereof, in order to its being communicated to you; and, if the Council ſhould think proper, of its being recorded in the Philoſophical Tranſactions, as I think it highly deſerves. In my opinion, a fact ſo extraordinary ſhould not be forgotten; as it may poſſibly be applied in no inconſiderable degree to public utility: if it ſhould not, the experiment itſelf, ſo ſucceſsfully conducted, is a deſirable thing to be known.

Mr. Charles Miller is a very ingenious perſon, and an excellent naturaliſt. He is the ſon of our worthy brother

brother Mr. Philip Miller, from whofe knowledge of, and publications in, botany, agriculture, and gardening, the public has received very great information and advantage. In confequence of my defire, Mr. Charles Miller has informed me, that having made, in the autumn of 1765, and in the fpring of 1766, an experiment of the divifion and tranfplantation of wheat, by which near two thoufand ears were produced from a fingle grain; and he having reafon to think, from the fuccefs attending this experiment, that a much greater quantity might be produced, he determined to repeat the experiment next year.

Accordingly, on the fecond of June, 1766, he fowed fome grains of the common red wheat; and, on the eighth of Auguft, which was as foon as the plants were ftrong enough to admit of a divifion, a fingle plant was taken up, and was feparated into eighteen parts. Each of thefe parts was planted again feparately. Thefe plants having pufhed out feveral fide fhoots by about the middle of September, fome of them were then taken up, and divided; and the reft of them between that time and the middle of October. This fecond divifion produced fixty feven plants.

Thefe plants remained through the winter; and another divifion of them, made between the middle of March and the twelfth of April, produced five hundred plants. They were then divided no further, but permitted to remain.

The plants were in general ftronger than any of the wheat in the fields. Some of them produced upwards of an hundred ears from a fingle root. Many of the ears

measured

meaſured ſeven inches in length, and contained between ſixty and ſeventy grains.

The whole number of ears, which by the proceſs beforementioned were produced from one grain of wheat, was twenty one thouſand one hundred and nine, which yielded three pecks and three quarters of clear corn; the weight of which was forty ſeven pounds, ſeven ounces; and, from a calculation made by counting the number of grains in one ounce, the whole number of grains might be about five hundred and ſeventy ſix thouſand eight hundred and forty.

By this account we find, that there was only one general diviſion of the plants made in the ſpring. Had a ſecond been made, the number of plants, Mr Miller thinks, would have amounted, at leaſt, to two thouſand, inſtead of five hundred; and the produce have been much enlarged. For he found by the experiment made the preceding year, in which the plants were divided twice in the ſpring, that they were not weakened by the ſecond diviſion. He mentions this to ſhew, that the experiment was not puſhed to the utmoſt.

The ground, in which this experiment was made, is a light blackiſh ſoil upon a gravelly bottom, and conſequently a bad ſoil for wheat. One half of the ground was very much dunged; the other half was not prepared with dung, or any other manure: no difference was however diſcoverable in the vigour or growth of the plants, nor was there any in their produce.

Mr. Miller adds, that he omits making any conjectures of the probability of turning this experiment

to public utility in agriculture; as that, he hopes, may be better aſcertained by a more extenſive one, which he hopes to make next year. A gentleman, who aſſiſted him in making the experiment laſt year, has ſown half an acre of land with wheat, from which they expect to have ſufficient to plant four acres next ſpring. The ſucceſs of this experiment they propoſe to tranſmit to me, when it is compleated; and of this, in due time, I ſhall not fail to inform you.

I am, Gentlemen,

Your moſt obedient

humble ſervant

W. Watſon.

XXXII. *Of the Theory of Circulating Decimal Fractions. By* John Robertſon, *Lib. R. S.*

Read Nov. 24, 1768.

THE great advantages ariſing from the uſe of Arithmetic, particularly in philoſophy and commerce, is ſufficiently known; therefore, every ſtep taken towards its perfection, has always been countenanced by thoſe who were beſt acquainted with its nature and value. On theſe motives I have been induced to offer the annexed paper to this learned Society.

Regiomontanus, it is ſaid, firſt among Europeans, added to the then known arithmetic, an operation by decimal fractions; which he exemplified in his triangular table. Its utility was readily ſeen, and embraced in many nations, and particularly in this; where it appears to have been cultivated in its theory, and facile modes of operation, more than in other places.

Many writers have remarked its excellency in numeral computation, and have pointed out compendiums to avoid the trouble of writing down ſuperfluous figures; particularly in the operations with concrete numbers, or thoſe relative to money, weight, and meaſure; where the gradations from one denomination to another do not proceed in an uniform progreſſion.

In finding the decimal values of the fractional parts of concrete, and other numbers, it often hap-

 pens,

pens, that thoſe decimals do not terminate, or end, with a few figures only; and ſometimes are infinite, or never end; and among theſe are many which have one or more figures conſtantly recurring; as in the following proportions, *viz.*

3 : 2 : : 1,0000, &c. : 0,6666, &c.
and 12 : 5 : : 1,0000, &c. : 0,4166, &c.
alſo 7 : 3 : : 1,0000, &c. : 0,428571,428571, &c.

In operations, with ſuch recurring decimal fractions, particularly in multiplication and diviſion, the work will either be longer than neceſſary, or be very inaccurate, if the numbers are not conſidered as circulating ones: and to come at the true reſults of ſuch operations, ſeveral authors have given preciſe rules; and ſome of them have ſhewn the principles upon which thoſe rules were founded.

In the annexed paper thoſe principles are, endeavoured to be, exhibited in a different, and in a more general and conciſe manner, than has hitherto been ſhewn: but the modes of working are not here annexed, as they are to be found in *Cunn*, *Malcolm*, *Marſh*, and others; and may hereafter be fully exemplified in a treatiſe of Arithmetic, by the author hereof, conſidered in a more mathematical order, than what has hitherto been appropriated to this moſt uſeful ſcience.

General Principles.

1. Number is ſuppoſed to begin at unity, and from thence to aſcend and deſcend: thoſe terms aſcending above unity, are integers; and thoſe deſcending below unity, are fractions.

When

When in the aſcending and deſcending parts of the ſcale, the gradation proceeds by a tenfold value from the right hand towards the left, the rank of numbers, thus generated, is called the decimal ſcale.

As every place in this ſcale is ten times the value of its next right hand place; therefore the firſt place in thefractional part, is $\frac{1}{10}$ of the place of units; and the ſecond, third, fourth, &c. deſcending places in the fractional part, is $\frac{1}{100}$, $\frac{1}{1000}$, $\frac{1}{10000}$, &c. part of the place of units.

Therefore every decimal fraction is equal to a ſeries ariſing from m ultiplying the firſt, ſecond, third, fourth, &c. terms of the decreaſing geometrical progreſſion $\frac{1}{10}+\frac{1}{100}+\frac{1}{1000}+\frac{1}{10000}$, &c. by the firſt, ſecond, third, and fourth, &c. terms in the given fraction reſpectively.

Thus. Let the given fraction be 0,3587 or $\frac{3587}{10000}$.

$$\text{Then } 0{,}3587=\frac{1}{10}\times 3+\frac{1}{100}\times 5+\frac{1}{1000}\times 8+\frac{1}{10000}\times 7$$
$$=\frac{3}{10}+\frac{5}{100}+\frac{8}{1000}+\frac{7}{10000}$$
$$=\frac{}{10000}+\frac{500}{10000}+\frac{80}{10000}+\frac{7}{10000}=\frac{3587}{10000}.$$

2. Every decimal fraction ariſes from diviſion, when the dividend is leſs than the diviſor.

For, diviſor : dividend :: 10 : firſt term of the fraction;
:: 100 : ſum of the firſt and ſecond terms;
:: 1000 : ſum of the firſt, ſecond, and third, terms, &c.

And according to the ratio of the diviſor to the dividend, the quotient, or decimal fraction, will be finite or infinite.

3. Among thoſe decimal fractions which are infinite, or do not end, ſome of them *recur*, or circulate; that is, the ſame figure or figures run over again and again *ad infinitum*.

As 0,333 &c.; 0,2323 &c.; 0,758758 &c.; 0,999 &c.

Here 0,333 &c. $=\frac{3}{10}+\frac{3}{100}+\frac{3}{1000}+\frac{3}{10000}$, &c.

0,2323 &c. $=\frac{2}{10}+\frac{3}{100}+\frac{2}{1000}+\frac{3}{10000}$, &c.

0,785785 &c. $=\frac{7}{10}+\frac{8}{100}+\frac{5}{1000}+\frac{7}{10000}+\frac{8}{100000}+\frac{5}{1000000}$, &c.

0,999, &c. $=\frac{9}{10}+\frac{9}{100}+\frac{9}{1000}+\frac{9}{10000}$, &c.

irculating fraction 0,999, &c. is equal to 1,0.
he difference between 1,0 and 0,999 &c. is less than can be

e a repetition of figures, it is usual to mark the first and last of g expressions, with points over the figures.

333 &c. is wrote $0,\dot{3}$
2323 &c. $0,\dot{2}\dot{3}$
785785 &c. $0,\dot{7}8\dot{5}$

e circulating decimal fractions are those which have each the same of circulating places; and begin to recur each at the same name.
finite decimal fraction may be considered as infinite; cyphers being he circulating part.
place of a circulating expression may be taken as the first; observ- the number and order of the circulating places be not altered.
the decimal fraction arises by division; if either place of the re- figures be taken for the first, the others will from thence re- circulate.
e several unlike circulating decimal fractions may be made to begin at places of like names.

in the decimal scale 10, 100, 1000, 10000, 100000, 1000000, &c. d indefinitely, be selected any rank of equi-distant terms, such, atever term therein is taken for the first term, and the first term is e common ratio to the rest; then will the sum of the reciprocals of rms, be equal to the reciprocal of the number which is unity less first term.

$\frac{1}{10} + \frac{1}{100} + \frac{1}{1000} + \text{\&c.} = \frac{1}{9}$; 10 being the 1st term.
$\frac{1}{100} + \frac{1}{10000} + \frac{1}{1000000} + \text{\&c.} = \frac{1}{99}$; 100 being the 1st term.
$\frac{1}{1000} + \frac{1}{1000000} + \frac{1}{1000000000} + \text{\&c.} = \frac{1}{999}$; 1000 being the 1st term.

For

$$\text{For } \frac{1}{10} + \frac{1}{100} + \frac{1}{1000} + \&c. = \frac{100+10+1}{1000} = \frac{111\ \&c.}{1000\ \&c.}$$

$$\frac{1}{100} + \frac{1}{10000} + \frac{1}{1000000} + \&c. = \frac{10000+100+1}{1000000} = \frac{10101\ \&c.}{1000000\ \&c.}$$

$$\frac{1}{1000} + \frac{1}{1000000} + \frac{1}{1000000000} + \&c. = \frac{1000000+1000+1}{1000000000} = \frac{1001001}{1000000000\ \&c.}$$

$$\text{But } 111\ \&c. \times 9 = 1000\ \&c. \text{ (by } 3^d\text{) Then } \frac{111\ \&c.}{1000\ \&c.} = \frac{1}{9}$$

$$10101\ \&c. \times 99 = 100000\ \&c. \quad \text{Then } \frac{10101\ \&c.}{1000000\ \&c.} = \frac{1}{99}$$

$$1001001\ \&c. \times 999 = 1000000000\ \&c. \quad \text{Then } \frac{1001001\ \&c.}{1000000000\ \&c.} = \frac{1}{999}$$

Hence the reciprocal of a number confifting of n places of 9's, is equal to a circulating number of n places, the right hand figure being 1, and the reft 0's.

Thus, $\frac{1}{9}$ $\frac{\quad}{1000\ \&c.}$

$$= \frac{10101\ \&c.}{1000000\ \&c.} \quad 0,\dot{0}\dot{1}$$

$$= \frac{1001001\ \&c.}{1000000000\ \&c.} = 0,\dot{0}0\dot{1}$$

$$= \frac{10001001\ \&c.}{1000000000000\ \&c.} = 0,\dot{0}00\dot{1}$$

6. If the reciprocal of a number confifting of n places of 9's, be multiplied by a number D, not exceeding n places; the product will be a circulating decimal fraction of n places, the right hand ones being the fame digits as are in the number D.

Let D = 3; or D = 23; or D = 785; or to any other number.

 Now

$\frac{1}{9} = \frac{111\ \&c.}{1000\ \&c.}$; $\frac{1}{99} = \frac{10101\ \&c.}{1000000\ \&c.}$; $\frac{1}{999} = \frac{1001001\ \&c.}{1000000000\ \&c.}$ (by 5th)

ore $\frac{1}{9}$, or $\frac{111\ \&c.}{1000\ \&c.} \times 3 = \frac{333\ \&c.}{1000\ \&c.} = 0{,}\dot{3}$

$\frac{1}{99}$, or $\frac{10101\ \&c.}{1000000\ \&c.}$ $\left\{\begin{array}{l} \times\ 3 = \frac{30303\ \&c.}{1000000\ \&c.} = 0{,}\dot{0}\dot{3} \\ \times\ 23 = \frac{232323\ \&c.}{1000000\ \&c.} = 0{,}\dot{2}\dot{3} \end{array}\right.$

$\frac{1}{999}$, or $\frac{1001001\ \&c.}{1000000000\ \&c.}$ $\left\{\begin{array}{l} \times\ 3 = \frac{3003003\ \&c.}{1000000000\ \&c.} = 0{,}\dot{0}0\dot{3} \\ \times\ 23 = \frac{23023023\ \&c.}{1000000000\ \&c.} = 0{,}\dot{0}2\dot{3} \\ \times\ 785 = \frac{785785785\ \&c.}{1000000000\ \&c.} = 0{,}\dot{7}8\dot{5} \end{array}\right.$

nce every circulating decimal fraction will be equivalent to a vulgar wherein the numerator is those circulating figures, and the denominsists of as many 9's, as are figures in the numerator.

$0{,}\dot{3} = \frac{3}{9}$. For $\frac{1}{9} \times 3 = \frac{3}{9} = 0{,}\dot{3}$ (by 6th)

$0{,}\dot{0}\dot{3} = \frac{3}{99}$ $\qquad \frac{1}{99} \times 3 = \frac{3}{99} = 0{,}\dot{0}\dot{3}$

$0{,}\dot{2}\dot{3} = \frac{23}{99}$ $\qquad \frac{1}{99} \times 23 = \frac{23}{99} = 0{,}\dot{2}\dot{3}$

ence a circulating decimal fraction, of any number of places, being ed by a number of as many 9's, will give a finite expression, having figures as are in the circulating one.

hus $0{,}\dot{6} \times 9 = 6$. For $9 : 1 :: 6 : 0{,}\dot{6}$

$0{,}\dot{0}\dot{6} \times 99 = 6$. $\qquad 99 : 1 :: 6 : 0{,}\dot{0}\dot{6}$

$0{,}\dot{2}\dot{5} \times 99 = 25$. $\qquad 99 : 1 :: 25 : 0{,}\dot{2}\dot{5}$

$0{,}\dot{6}2\dot{5} \times 999 = 625$. $\qquad 999 : 1 :: 625 : 0{,}\dot{6}2\dot{5}$

Hence

Hence it appears, that, in common multiplication, the product of a circulating number, by its proper denominator, in 9's, will be deficient of the true product by that circulating number.

Thus $\dot{0},\dot{6} \times 9 = 5,4$; then $5,4 + ,6 = 6$. For $\frac{6}{9} = 0,\dot{6}$

$0,0\dot{6} \times 99 = 5,94$; then $5,94 + 0,06 = 6$. For $\frac{6}{99} = 0,0\dot{6}$

$0,\dot{6}2\dot{5} \times 999 = 624,375$; then $624,375 + 0,625 = 625,0$

Hence. Any finite number is in proportion to the same number recurring, as the proper denominator of the circulate is to that denominator increased by unity.

Thus $9 : 10 :: 6 : \dot{6}$. For $\dot{6} \times 9 = 6 \times 10$

$99 : 100 :: 25 : \dot{2}\dot{5}$. For $\dot{2}\dot{5} \times 99 = 25 \times 100$.

SCHOLIUM.

If to the preceding articles, be joined the compendiums of multiplying and dividing by any number of 9's, they will constitute the whole of the theory, upon which depend all the operations with circulating numbers: for as these have 9's for their denominator, wanting unity in the lowest place to make them 10's; therefore unity for every 9 is applied in some additions and multiplications: Or, the circulating parts being reduced to finite number; then working with them by the common rules, will give finite results; which results are to be reduced to circulates by contrary operations to what were used to reduce the circulates to finites.

XXXIII. *A Letter from Mr.* J. R. Forster, *F. A. S. to* M. Maty, *M. D. Sec. R. S. containing some Account of a new Map of the River* Volga.

Warington, October 24, 1768.

S I R,

Read Nov. 8, 1768.

I Received both your favours of the 1st and 17th instant, in due time.

I am very glad the committee of the Royal Society has done my map of the river Volga * the honour of having it engraved and published in their Transactions.

You wished to be informed of the manner in which it was constructed. I readily comply with your desire, and send you the following short account, which I hope will be satisfactory.

At my arrival at Saratof, which is the chief town of the district given by her Russian majesty to the German colonies, I got two MSS. maps in Russian characters, done by Mr. Reuss, a major, and able engineer, in the Russian service, who went along the river Volga, from Saratof to Tsaritsin, by water; and, at every winding of the river, went on shore to take the angles, and to measure the meadows beyond the Volga; this map was upon five sheets, pasted together. The other MS. map had the same author, consisted of eight sheets, and described the country from Petrofsk and Saratof, to Tsaritsin, along the Volga, and westward to the rivers Choper and Don. It was constructed upon an actual survey, in which the major was assisted by three engineer officers; however,

* See TAB. IX, p. 216.

as

as this ſurvey had been made in a hurry, by meaſuring the roads on horſeback, with a long rope of ten poles, and taking the angles only from village to village, I cannot depend upon it as entirely correct.

The country from Saratof to Dmitrefsk I paſſed through myſelf; I meaſured one baſe, and then took, with an inſtrument, the angles of the moſt ſtriking objects, as hills, villages, and rivers, on both ſides of the Volga. On the eaſtern ſide of that river, I got as far as the lake Yelton, and the ſandy deſart Ryn, always uſing the ſame method: from thence I went to the river Yerooſlan, almoſt to its very ſource, and came back again, to the place where it enters the Volga; from thence I proceeded to Saratof, along the Volga, rectifying Mr. Reuſs's angles and maps; and, beſides, I made an excurſion 30 miles above Saratof, on the eaſt ſhore of the Volga. All the places, which are marked with lines in my map, are either colonies already ſettled, or places intended to be filled with coloniſts, who were then on their march from Peterſburgh towards Saratof. When I preſented this map to the Academy of Sciences at Peterſburg, it was looked upon as an Unique; for they had in the geographic department not one map of this country, although more than 3000 MSS. maps and drawings were kept in their portefolios. I can, therefore, very juſtly call this the firſt tolerable map; and whoever will take the trouble of comparing my work with Mr. Hanway's, or Olearius's map, will eaſily ſee the remarkable difference between them. Mr. Hanway went in a great hurry over this part of the country, and had no inſtruments, nor did he make any ſurvey; he was alſo unacquainted with the language of the country; the

the knowledge of which is of great importance to one who would succeed in an undertaking of this nature. Olearius never ſtirred out of his ſhip: and the map contained in the Ruſſian Atlas, is ſo ſhamefully deficient and faulty, that the academy very readily acknowledged the improvements made by my map, and thankfully accepted of it, although no uſe was made of it. During my ſtay at London, in 1766, I made a reviſion of my papers, calculated again all diſtances and angles, and corrected my map in more than twenty places, ſo that this may be juſtly called a new and improved one.

The canal made by Perry, in the year 1697–1701, for the conjunction of the Volga and Don, I ſurveyed; and conſtructed a ſpecial map of it, of more than three yards ſize, and can therefore anſwer for its accuracy. The canal begun by the Turks, nobody ever took notice of beſides me in a map; which I did, from an account I got, by means of Mr. Reuſs, and ſome others, and from a rough ſketch of it, communicated to me. That part of the Don, which appears in the map, is taken from the very accurate and famous map made by the vice-admiral Cornelius Cruys, and publiſhed in fourteen ſheets in Holland.

This, I hope, will ſatisfy the public in reſpect to the accuracy of the performance, and enable the Society to judge better, whether it deſerves to be publiſhed or not. I am, with the greateſt reſpect,

SIR,

Your moſt obliged and humble ſervant,

John Reinhold Forſter.

XXXIV. *An*

Received October 3, 1768.

XXXIV. *An Account of the Lymphatic Syſtem in Birds; by Mr.* William Hewſon, *Reader in Anatomy: In a Letter to* William Hunter, *M. D. F. R. S. and by him communicated to the Society.*

S I R,

Read December 8, 1768.

HAVING been ſo fortunate, in a ſeries of experiments made with that view, as to trace out the lymphatic ſyſtem in birds, I have ventured to offer the following account of it to you, in order to be preſented, if you think proper, to the Royal Society; and, I flatter myſelf, this diſcovery will be looked upon as ſome acquiſition to phyſiology.

The lymphatic ſyſtem has been ſuppoſed to be wanting in birds; and abſorption in that kind of animals to be carried on by branches of the common veins. Phyſiologiſts were led into this opinion by obſerving, that though the lacteals and meſenteric glands were eaſily ſeen even in the ſmalleſt quadruped, yet the moſt acute anatomiſts had not been able to find in any bird the leaſt appearance either of thoſe veſſels or glands. The difficulty of diſcovering the lacteals in birds was, no doubt, principally owing to the tranſparency, or want of colour, in the fluid which they contain. In quadrupeds the lacteals are eaſily found, as they are filled with chyle, which is moſtly opaque and white; whereas, in birds, the chyle is as pellucid and colourleſs as the veſſels themſelves.

 The

The want of meſenteric glands was another cauſe of our remaining ſo long ignorant of thoſe veſſels.

This ſyſtem may be divided in birds, as it is in quadrupeds, into the branches, viz. the lacteals and lymphatics, and their trunk, or thoracic duct. The lacteals indeed, in the ſtricteſt ſenſe, are, in birds, the lymphatics of the inteſtines, and like the other lymphatics carry a tranſparent lymph. And inſtead of one thoracic duct there are two, of which one goes to each jugular vein. In theſe circumſtances it would ſeem, that birds differ from quadrupeds, ſo far at leaſt as I may judge from the diſſection of a gooſe, which was the bird I choſe as moſt proper for this enquiry.

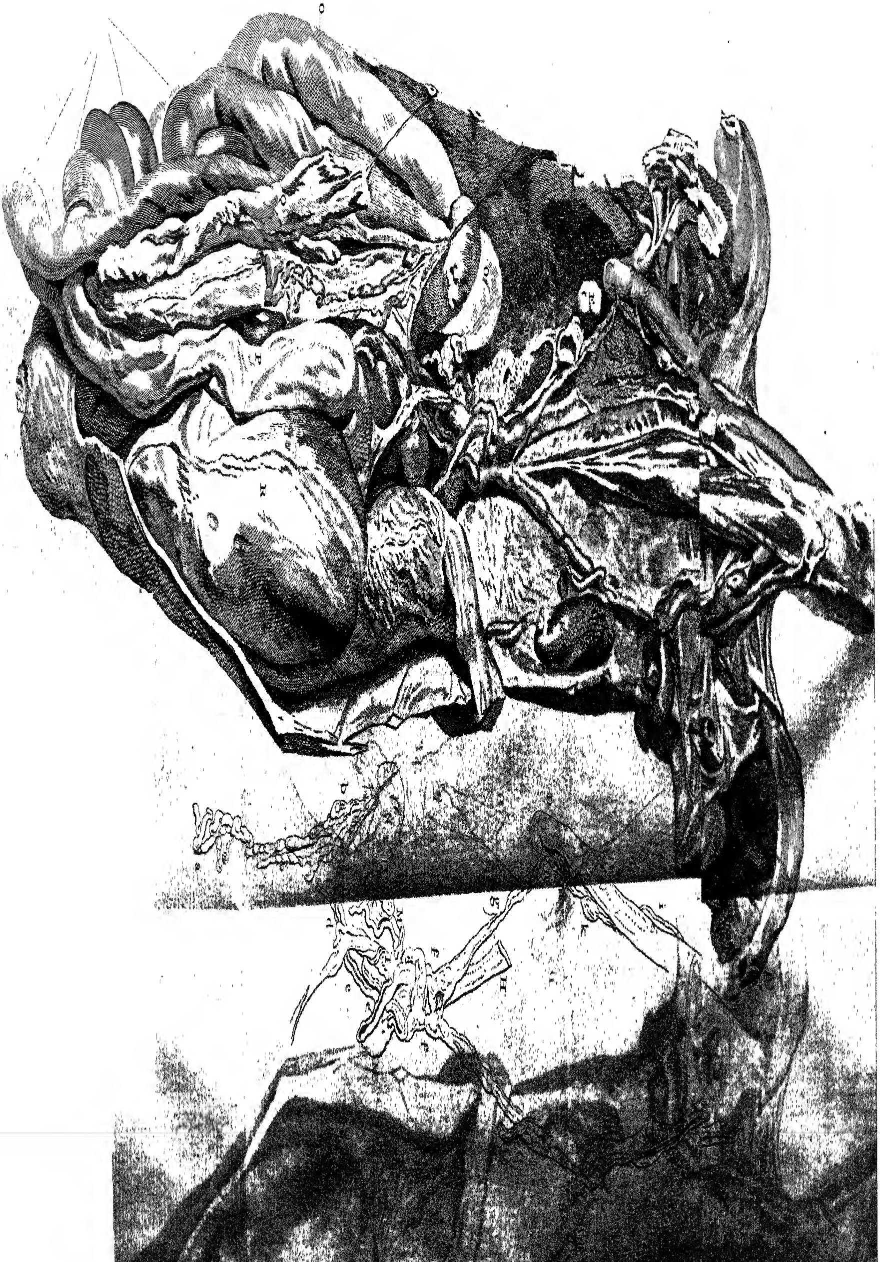
So much being premiſed, I ſhall next give a deſcription of what I have ſeen of thoſe veſſels in this fowl; and to illuſtrate the deſcription I ſhall add a figure from the ſame ſubject, in which thoſe veſſels were filled with quick-ſilver.

The lacteals run from the inteſtines upon the meſenteric veſſels. Thoſe of the *duodenum (a)* * paſs by the ſide of the *pancreas (Q)* †, and probably receive its lymphatics: afterwards they get upon the cœliac artery, of which the ſuperior meſenteric is a branch. Whilſt they are upon this artery they are joined by the lymphatics from the liver *(b)*: here they form a plexus, which ſurrounds the cœliac artery *(cc)*; at this part they receive a lymphatic from the gizzard *(d)*; and a little farther, another from the lower or glandular part of the *œſophagus (e)*. Having now got to the root of the cœliac artery, they are joined by the lymphatics from the renal *capſulæ*; and near the ſame part, by the

* See Plate X. in the outlines.

† See the ſame Plate in the Figure itſelf.

lacteals



lacteals from the other ſmall inteſtines, which veſſels accompany the lower meſenteric artery. Theſe laſt mentioned lacteals, before they join thoſe from the *duodenum*, receive from the *rectum* a lymphatic, which runs with the blood-veſſels of that gut. Into this lymphatic ſome ſmall branches from the kidneys ſeem to enter, which coming from thoſe glands upon the meſentery of the *rectum*, at laſt open into its lymphatics. At the root of the cœliac artery, the lymphatics of the lower extremities probably join thoſe from the inteſtines. The former I have not yet traced to their termination, though I have diſtinctly ſeen them on the blood-veſſels of the thigh; and in one ſubject, which I injected, ſome veſſels were filled, contrary to the courſe of the lymph, from the network near the root of the cœliac artery; theſe veſſels ran behind the *cava*, and down upon the *aorta*, near to the origin of the crural arteries, and I preſume they were the trunks of thoſe branches which I had ſeen in the thigh. At the root of the cœliac artery, and upon the contiguous part of the *aorta*, a network *(f f)* is formed by the lacteals and lymphatics above deſcribed. This network conſiſts of three or four tranſverſe branches, which make a communication between thoſe which are lateral. In the ſubject from which my drawing was made, there were four. From this network ariſe the two thoracic ducts *(g g)*; of which one lies on each ſide of the ſpine, and runs upon the lungs obliquely upwards towards the jugular vein, into which it opens (*l* & *n*); not indeed into the angle between the jugular and ſubclavian, as in the human ſubject, but into the inſide of the jugular vein, nearly oppoſite to that angle. The thoracic duct of the left ſide is joined

 by

by a large lymphatic *(h)*, which runs upon the *œſophagus*, and can be traced as far as the lower or glandular part of that canal; from which part, or from the gizzard, it ſeems to iſſue. The thoracic ducts are joined by the lymphatics of the neck (and probably by thoſe of the wings) juſt where they open into the jugular veins.

The lymphatics of the neck * generally conſiſt of two pretty large branches, on each ſide of the neck, accompanying the blood-veſſels. Thoſe two branches join near the lower part of the neck; and the trunk is, in general, as ſmall, if not ſmaller, than either of the branches. This trunk runs cloſe to the jugular vein *(i i)*, gets on its inſide, and then opens into a lymphatic gland *(k k)*. From the oppoſite ſide of this gland, a lymphatic comes out, which pours the lymph into the jugular vein. On the left ſide, the whole of this lymphatic joins the thoracic duct of the ſame ſide, *(l)*; but, on the right, one part of it goes into the inſide of the jugular vein a little above the angle *(m)*, whilſt another joins the thoracic duct, and with that duct forms a common trunk, which opens into the inſide of the jugular vein, a little below the angle which that vein makes with the ſubclavian *(n)*.

To this deſcription it may be neceſſary to add, that though it be taken from one ſubject, yet in three others of the ſame ſpecies which I examined carefully, I ſaw nothing which diſagreed with it. I particularly attended to the number of the thoracic ducts, ſuſpecting, that poſſibly in this ſubject, the two that I had ſeen might

* It is but doing juſtice to the ingenious Mr. John Hunter, to mention here, that theſe lymphatics in the necks of fowls were firſt diſcovered by him many years ago.

be

be only a variety, which is a circumſtance that, as we are told, has occurred even in the human body. But in three others of this ſpecies, which I likewiſe ſucceſsfully injected, I ſtill ſaw two ducts; and therefore I am inclined to believe, that this is the conſtant number. I likewiſe carefully attended to the veſſels coming from the gland on the right ſide: and in the only two ſubjects in which the lymphatics of the neck were properly filled, I obſerved, that one part of it opened immediately into the vein, and the other joined the thoracic duct of that ſide; whilſt, on the left ſide, the veſſel which iſſued from the gland wholly joined the thoracic duct. In all the four ſubjects I evidently ſaw that the thoracic ducts open into the inſide of the jugular veins.

This ſyſtem in birds differs moſt from that in quadrupeds in the following pariculars. 1ſt, In the chyle being tranſparent and colourleſs. 2dly, In there being no viſible lymphatic glands, neither in the courſe of the lacteals, nor in that of the lymphatics of the *abdomen*, nor near the thoracic ducts. 3dly, In the ſeveral parts of this ſyſtem in birds being more frequently enlarged, or varicoſe, than in quadrupeds. In particular, this appears to be the caſe of the veſſels which conſtitute the network at the root of the cœliac artery in that ſubject from which the drawing was taken. The lacteals are frequently enlarged in ſome places; ſo are the thoracic ducts; and the lymphatics on each ſide of the neck are commonly, when taken together, larger than their trunk which opens into the lymphatic gland. In one ſubject, where inſtead of two lymphatics on the left ſide I found only one, that veſſel was as large as a crow-quill; whilſt the

lower part of it, which entered the gland, was much ſmaller.

Thus far the account of what I ſaw: I ſhall next beg leave to obſerve, that, as the ſuppoſed want of this ſyſtem in birds has been conſidered as a ſtrong argument in favour of abſorption by the common veins, now, ſince we find it not wanting, that theory muſt be much weakened. And I may likewiſe add, that abſorption ſeems to be carried on in birds, as in quadrupeds, by this ſyſtem, at leaſt principally; indeed I am inclined to believe, entirely; for no arguments brought in favour of abſorption by the common veins appear to me of equal validity with thoſe that can be urged againſt it. The contrary opinion is indeed embraced by the moſt learned and acute phyſiologiſt of the preſent age, who, treating of this ſubject, expreſſes himſelf in the following manner; "It is a ſtrong argument in favour of abſorption by "the common veins, that neither birds, amphibious "animals, nor fiſh with cold blood, have either the "lacteal or the lymphatic ſyſtem. Nature common-"ly obſerves a pretty ſtrict analogy in her works, and "makes uſe of ſimilar organs to perform ſimilar func-"tions. Now in all animals, quadrupeds and the "whale excepted, we muſt admit of abſorption by "the meſenteric veins, if in thoſe animals there is "no other way for the chyle to get into the blood. "And if thoſe veins in birds and amphibious animals "abſorb the chyle, it is very probable they likewiſe "abſorb it in quadrupeds, in which they equally "exiſt." But the exiſtence of this ſyſtem in birds is not the only fact which might be adduced to invalidate the above opinion; for I have ſeen a part of it very diſtinctly in one of the *amphibia*, viz. the

Turtle

Turtle *. Whether it is to be found in fiſh, I cannot yet determine. Since I ſaw it in birds and in the Turtle, I made indeed ſome enquiries after it in fiſh, but hitherto without ſucceſs. Yet, that they are not without ſuch veſſels, I think is probable, from conſidering that the lymphatics are ſo general, as to be found in quadrupeds, birds, and amphibious animals. And from the conſideration of the extenſiveneſs of this ſyſtem through ſo many claſſes of animals, I am inclined to think that opinion moſt probable, which you advanced ſome time ago, when you printed your diſcovery of the uſe of thoſe veſſels, viz. "That the lymphatics are the "*only* abſorbents †".

For the ſake of thoſe who may incline to proſecute this enquiry farther, I ſhall now relate the method by which theſe veſſels may be demonſtrated; and that is, having choſen a young and very lean gooſe, and fixed it upon a table, let the abdomen be opened whilſt it is yet alive, and a ligature be paſſed round its meſenteric veſſels, as near the root of the meſentery as poſſible. The lacteals will begin to appear near the ligature in

* The part of this ſyſtem, which I ſaw in the Turtle, was the lacteals. I filled them with quick-ſilver as far as the root of the meſentery, where they formed a conſiderable net-work into which a lymphatic of the ſpleen entered. I had not an opportunity of tracing them farther, having taken the meſentery out of the animal before I had thought of looking for theſe veſſels, as I was not at that time intent on this enquiry. The lacteals in that animal agreed with thoſe in the bird above deſcribed, in not having any meſenteric glands. From this circumſtance, and from another obſervation which I made, I am inclined to believe, that the whole ſyſtem in this animal will be found to agree pretty exactly with that of birds.

Theſe veſſels I obſerved ſo long ago as in the winter 1763-64.

† Vide Hunter's Commentaries, ch. v.

a few minutes after it is made, eſpecially if the bird has been well fed three or four hours before the experiment. The lymphatics in the neck may be ſhewn in the ſame manner; that is, by making a ligature on the jugular vein at the lower part of the neck; and to be more certain of including the lymphatics, which are near it, we muſt take care not to paſs the needle too cloſe to that veſſel. When they are to be injected, they muſt be opened at a convenient part, and a proper pipe fixed in them for that purpoſe.

For the greater ſatisfaction of thoſe who may think this paper worthy their attention, I have prepared two birds, whoſe lymphatic ſyſtems are filled with quick-ſilver, in order to be compared with the figure: theſe have already been ſhewn to ſeveral members of the learned Society, who honoured me with their preſence whilſt the ſubjects were freſh; and who, I flatter myſelf, were then ſatisfied with the exactneſs of the drawing.

Mr. Hewſon begs leave to add, that ſince the above paper on the lymphatic ſyſtem in birds was put into the hands of the ſecretary of the Royal Society, he has diſcovered the ſame ſyſtem in fiſh; and has likewiſe been ſo fortunate as to procure a Turtle, whoſe lymphatic ſyſtem he has traced out, and has got delineated. An account of thoſe diſſections, with the figures, he intends ſoon to have the honour of laying before the Society.

Windmill Street, Dec. 3,
1768.

Expla-

Explanation of PLATE X.

N. B. *The ſmall Letters refer to the Outlines, and the Capital Letters in general refer to the Figure, except where the contrary is ſpecified.*

A	The Neck.
BB	The Clavicle divided near its middle.
C	The left Subclavian Artery.
DD	The Jugular Veins.—See the Outlines.
EE	The Pulmonary Arteries.
FF	The two Branches of the Trachea.
GG	The Lungs.
H	The Aorta—in the Outlines.
I	The Cœliac Artery—in the ſame.
L	The Oeſophagus turned to a ſide.
MM	The Renal *Capſulæ*, or *Glandulæ Renales*—in the Outlines.
N	A ſmall part of the Liver, fixed to a Rib by a thread.—In the Outlines.
OOO	Inteſtines.
P	The *Duodenum.*
Q	The *Pancreas* fixed to a Rib by a thread.
R	The Gizzard.

a	The Lacteals which come from the *Duodenum.*
b	The Lymphatics of the Liver.
cc	A *Plexus* formed by the above mentioned Lacteals and Lymphatics, which ſurrounds the Cœliac Artery.
d	A Lymphatic from the Gizzard.

e A Lymphatic from the lower part of the *Oeso-phagus.*

ff A Network formed by the Lymphatics upon the *Aorta.*

gg The two Thoracic Ducts.

ii The Trunks of the Lymphatics of the Neck.

kk The Glands through which the Lymphatic Veſſels of the Neck paſs. That of the left ſide is oblong, and could not well be repreſented in a Figure.

l The Thoracic Duct of the Left Side, and the Lymphatic Veſſel of the Neck, opening together into the inſide of the Jugular Vein.

m A part of the Lymphatic veſſel of the right ſide of the neck, opening into the Jugular Vein.

n The Thoracic Duct of the right ſide, joined by a part of the Lymphatic Veſſel of the neck, and then opening into the inſide of the Jugular Vein.

XXXV. *A Catalogue of the* Fifty Plants *from* Chelſea Garden, *preſented to the* Royal Society *by the worſhipful Company of Apothecaries, for the Year* 1767, *purſuant to the Direction of Sir* Hans Sloane, *Bart. Med. Reg. et Soc. Reg. nuper Præſes: By* William Hudſon, *Societatis Regiæ & clariſſ. Societatis Pharmaceut.* Lond. *Soc. Hort.* Chelſean. *Præfectus et Prælector Botanici.*

Read Dec. 8, 1768.

2251 ALcea *ficifolia*, foliis palmatis. Lin. Sp. pl. 967.
Malvea roſea, folio ficus. Bauh. pin. 315.

2252 Allium *carinatum*, caule planifolia bulbifera, ſtaminibus ſubulatis. Lin. Sp. pl. 426.
Allium umbella bulbifera, vagina bicorni, foliis carinatis. Hall. all. 27. tab. I. f. 2.

2253 Amaranthus *ſpinoſus*, racemis pentandricis erectis, axillis ſpinoſis. Lin. Sp. pl. 1407.
Amaranthus Indicus ſpinoſus, ſpica herbacea. Herm. Lugdb. 31. tab. 33.

2254 Bidens *bipinnata*, foliis bipinnatis inciſis, calycibus involucratis, corollis ſemiradiatis, ſeminibus divergentibus. Lin. Sp. pl. 1166.
Chryſanthemum Americanum, coridis indi folio. Herm. Par. 123. tab. 123.

2255 Bidens *pilosa*, foliis pinnatis subpilosis, caulis geniculis barbatis, calycibus involucro simplici, seminibus divergentibus. Lin. Sp. pl. 1166.

Bidens latifolia, hirsutior, semine angustiore radiato. Dill. Hort. Elth. 51. tab. 43. f. 51.

2256 Campanula, *Erinus*, caule dichotomo, foliis sessilibus, utrinque dentatis, floralibus oppositis. Lin. Sp. pl. 240.

Erini s. Rapunculi minimum genus. Columm. phytob. 122. tab. 28.

2257 Capparis *spinosa*, pedunculis solitariis unifloris, stipulis spinosis, foliis annuis, capsulis ovalibus. Lin. Sp. pl. 720.

Capparis spinosa, fructu minore, folio rotundo. Bauh. pin. 480.

2258 Conyza *hirsuta*, foliis ovalibus, integerrimis scabris subtus hirsutis. Lin. Sp. pl. 1209.

2259 Coreopsis *lanceolata*, foliis lanceolatis integerrimis ciliatis. Lin. Sp. pl. 1283.

Bidens succisæ folio, radio amplo laciniato. Dill. H. Elth. 55. tab. 48. f. 56.

2260 Coreopsis *leucantha*, foliis pinnatis serratis, florum radio diversicolore. Lin. Sp. pl. 1282.

2261 Dianthus *glaucus*, floribus subsolitariis, squamis calycinis lanceolatis quaternis brevibus, corollis crenatis. Lin. Sp. pl. 588. Huds. Fl. Angl. 161.

2262 Dianthus *Carthusianorum*, floribus subaggregatis: squamis calycinis ovatis aristatis tubum subæquantibus, foliis sublinearibus trinerviis. Lin. Sp. pl. 586.

Cary-

Caryophyllus ſylveſtris vulgaris latifolius. Bauh. pin. 209.

2263 Fraxinus, *Ornus*, foliolis ſerratis, floribus corollatis. Lin. Sp. pl. 1510.

Fraxinus florifera botryoides, Moriſ. præl. 265. Hort. Angl. 33. tab. 9.

2264 Forſkohlea. Lin. Mantiſ. 72.

Chamædryfolia tomentoſa maſcatenſis. Pluk. alm. 97. tab. 275. f. 6.

2265 Geropogon *glabrum*, foliis glabris. Lin. Sp. pl. 1109.

Tragopogon gramineo folio, glabrum, flore dilute incarnato. Raj. Hiſt. 111. 149.

2266 Gnaphalium *undulatum*, herbaceum foliis decurrentibus, lanceolatis, acutis undatis ſubtus tomentoſis. Lin. Sp. pl. 1197.

Elichryſum graveolens acutifolium, caule alato. Dill. Hort. Elth. 130. tab. 108. f. 130.

2267 Hamellia *patens*, racemis patentibus. Lin. Sp. pl 246. Jacq. Amer. 16.

Periclymenum arboreſcens, ramulis inflexis; flore luteo. Plum. ic 218.

2268 Helenium *autumnale*. Lin. Sp. pl. 1248.

Aſter floridianus, caule alato. Pluk. Phyt. tab. 372. f. 9.

2269 Helicteres, *Iſora*, foliis cordatis ſerratis, fructu compoſito contorto. Lin. Sp. pl. 1367.

Hicteres, arbor Indiæ orientalis, ſiliqua varicoſa, et funiculi in modum contortuplicata. Pluk. Alm. 181. tab. 245. f. 2.

2270 Holoſteum *cordatum*, foliis ſubcordatis. Aman. ac. 3. p. 21. Lin. Sp. pl. 130.

Alsine Americana, nummulariæ folio. Herm. parad. 11. tab. 11.

2271 Holosteum *umbellatum*, floribus umbellatis. Loef. it. 120. Lin. Sp. pl. 130.

Lychnis graminea hirsuta umbellifera. Moris. Hist. 2. p. 546. s. 5. tab. 22. f. 46.

2272 Hypericum *tomentosum*, floribus trigynis: calycibus serrato-glandulosis, foliis semiamplexicaulibus flexuosis tomentosis, caulibus prostratis. Gouan. monsp. 402. Lin. Sp. pl. 1106.

Hypericum supinum tomentosum alterum. Clus. Hist. 2. p. 181.

2273 Lobelia, *Erinus*, caule patulo, foliis lanceolatis serratis, pedunculis longissimis. Lin. Sp. pl. 1320.

Campanula minor Africana, crinifacie, flore violaceo, caulibus erectis. Herm. Lugdb. 110. tab. 111.

2274 Loeflingia, *Hispanica*, Loef. it. 113. tab. 1. f. 2. Lin. Sp. pl. 50.

2275 Malva *scabrosa*, caule fruticoso, pilis simplicibus, foliis lobatis, floribus erectiusculis, petalis incumbentibus. Lin. Sp. pl. 968.

Malva Africana frutescens, flore rubro. Comm. hort. 2. p. 171. tab. 86.

2276 Malva *limensis*, caule erecto herbaceo, foliis lobatis, spicis secundis axillaribus, seminibus lævibus. Lin. Sp. pl. 968.

2277 Mimulus *ringens*, erectus, foliis oblongis linearibus sessilibus. Lin. Sp. pl. 884.

Digitalis perfoliata glabra, flore violaceo minore. Hist. Ox. 11. p. 475. s. 5. tab. 8. f. 6.

2278 Morus *papyrifera*, foliis palmatis, fructibus hiſpidis. Lin. Sp. pl. 1399.
Morus papyrifera, ſativa Japonica. Seb. Theſ. 1. p. 44. tab. 28. f. 3.

2279 Parietaria *Luſitanica*, foliis ovatis obtuſis, caulibus ſtriatis lævibus filiformibus procumbentibus. Lin. Sp. pl. 1492.
Parietaria Luſitanica, annua minima. Tourn. 506. Raj. Hiſt. III. p. 129.

2280 Paſſiflora *rubra*, foliis bilobatis cordatis acuminatis. Lin. Sp. pl. 1356.
Flos paſſionis, folii media lacinia quaſi abſciſſa, flore minore carneo. Sloan. Jam. 144. Hiſt. I. p. 229.

2281 Paſſiflora *ſerratifolia*, foliis indiviſis ſerratis. Lin. Sp. pl. 1355.
Granadilla Americana, folio oblongo leviter ſerrato, petalis ex viridi rubeſcentibus. Mart. cent. 36. tab. 36.

2282 Plantago *maritima*, foliis ſemicylindraceis integerrimis: baſi lanatis, ſcapo tereti. Lin. Sp. pl. 165. Hudſ. Angl.
Coronopus maritimus major. Bauh. pin. 190.

2283 Plantago *Africana*, caule ramoſo fruticoſo, foliis lanceolatis, dentatis capitulis aphyllis. Lin. Sp. pl. 168.
Pſyllium, foliis crenatis, Indicum. Bauh. pin. 191.

2284 Plantago *Cynops*, caule ramoſo fruticoſo, foliis filiformibus integerrimis ſtrictis, capitulis ſubfoliatis. Lin. Sp. pl. 167.
Pſyllium majus ſupinum. Bauh. pin. 191.

2285 Poly-

2285 Polycarpon *tetraphyllum*. Lin. Sp. pl. 131.
Anthyllis alsinifolia polygonoides majo. Barr. car. 103. tab. 534.

2286 Piercea *glabra*, foliis ovato-lanceolatis glabris. Mill Dict. Hort. ed. 8.
Rivina, floribus octandris dodecandrisve. Lin. Sp. pl. 117.

2287 Saxifraga *umbrosa*, foliis obovatis subretusis cartilagineo-crenatis, caule nudo paniculato. Lin. Sp. pl. 574.
Geum folio subrotundo minori pistillo floris rubro Mill. i. e. 141. f. 2.

2288 Sida *Indica*, foliis cordatis sublobatis stipulis reflexis, pedunculis longioribus, capsulis multilocularibus scabris calyce longioribus. Lin. Sp. pl. 964.
Abutilon Indicum. Comm. hort. 11. tab. 1.

2289 Silphium *perfoliatum*, foliis oppositis deltoidibus, petiolatis perfoliatis. Lin. Sp. pl. 1301.

2290 Silene *polyphylla*, foliis fasciculatis setaceis: ramorum florentium oppositis. Roy. Lugdb. 447. Lin. Sp. pl. 601.
Lychnis sylvestris VIII. Clus. Hist. I. p. 290.

2291 Sisymbrium *monense*, acaule, foliis dentato-pinnatis subpilosis. Lin. Sp. pl. [illegible]18. Huds. Angl.
Eruca monensis laciniata, flore luteo majore. Dill. Hort. Elth. 135. tab. 111. f. 135.

2292 Solanum *ferox*, caule aculateo herbaceo, foliis cordatis angulatis tomentosis, baccis hirtis calyce obtectis. Lin. Sp. pl. 267.

2293 So-

2293 Solanum *Virginianum*, caule aculeato herbaceo foliis pinnatifidis utrinque aculeatis: laciniis sinuatis obtusis, calycibus aculeatis. Lin. Sp. pl. 267.

Solanum Americanum laciniatum spinosissimum. Dill. Hort. Elth. 360. tab. 267. f. 346.

2294 Solanum *sodomeum*, caule aculeato fruticoso, foliis obovatis pinnatifido-sinuatis obtusis sparse aculeatis nudis, calycibus aculeatis. Lin. Sp. pl. 265.

Solanum spinosum, profunde laciniatis foliis, subtus lanuginosis, madaraspatanum. Pluk. tab. 316. f. 4?

2295 Solidago *rigida*, foliis caulinis ovatis scabris, ramis alternis fastigiatis, corymbis terminalibus. Lin. Sp. pl. 1235.

Virga aurea novæ Angliæ, lato rigidoque folio. Herm. par. 243. tab. 243.

2296 Sonchus *maritimus*, pedunculo nudo, foliis lanceolatis amplexicaulibus indivisis retrorsum argute dentatis. Lin. Sp. pl. 1116.

Sonchus angustifolius maritimus. Pluk. phyt. tab. 62. f. 5.

2297 Spermacoce *tenuior*, glabra, foliis linearibus, staminibus inclusis. Lin. Sp. pl. 147.

Spermacoce verticillis tenuioribus. Dill. Hort. Elth. 370. tab. 277. f. 359.

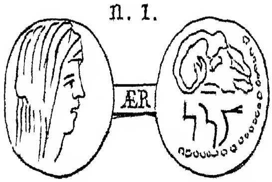
2298 Urtica *cylindrica*, foliis oppositis oblongis, amentis cylindricis solitariis indivisis sessilibus. Lin. Sp. pl. 1396.

Urtica racemosa humilior iners. Sloan. Jam. 38.

2299 Urtica *divaricata*, foliis alternis ovatis, racemis compositis divaricatis. Lin. Sp. pl. 1397.
Urtica racemosa major Virginiana mitior, s. minus urens. Pluk. phyt. 237. f. 2.

2300 Zinnia *pauciflora*, floribus paucis. Lin. Sp. pl. 1296.
Bidens calyce oblongo, seminibus radii corolla non decidua coronatis. Mill. tab. 64.

n. 1.



Penes Joannem Swinton, S.T.B.
Oxonienſ. R.S.S.

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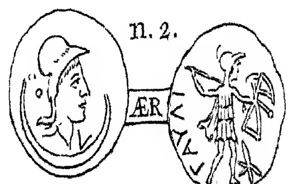

Apud Joannem Swinton, S.T.B.
Oxonienſ. R.S.S.

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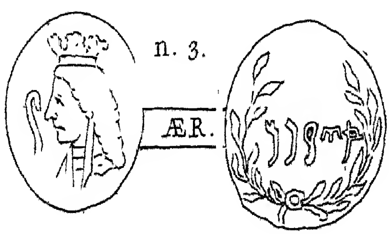
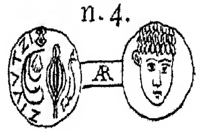


Apud Car. Godwyn, S.T.B.
Coll. Ball. Oxon. Soc.

n. 4.



Apud Joannem Swinton, S.T.B.
Oxonienſ. R.S.S.

Received September 29, 1768.

XXXVI. *Interpretation of the Inſcription on a* Punic *Coin, ſtruck in the Iſle of* Gozo, *never hitherto explained. In a Letter to* Charles Morton, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Cuſtos Archivorum of the Univerſity of* Oxford, *Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Good Sir,

Read Nov. 24, 1768.

IT has been obſerved by the learned Sig. Abate Ridolfino Venuti, that ſuch antient pieces as that before me (ſee TAB. XI. n. 5.), which has a place in my ſmall cabinet, adorned with a Punic inſcription on the reverſe (1), are not ſeldom found in the iſland of Malta. This ſeems in a good meaſure to have induced him to denominate them Malteſe (2) medals, and to conſider them as ſtruck in that iſland. On one ſide of my coin we diſco-

(1) *Saggi di Diſſertazioni Accàdemiche pubblicamente lette nella Nobil. Accademia Etruſca dell' antichiſſima Città di Cortona.* Tom. I. p. 35—42.

(2) *Diſſertaz. dell' Abat.* Ridolfin. Venut. in *Sag. di Diſſertaz. Accademich. &c.* ubi ſup.

ver the head of a woman veiled, and on the reverſe that of a ſheep, under which ſtands a word formed of three Punic characters. The piece itſelf is well enough preſerved, and the letters in particular have almoſt intirely eſcaped the injuries of time. Several ſimilar medals have been publiſhed by (3) Paruta, (4) Laſtanoſa, the (5) Marquis Scipio Maffei, (6) Sig. Abate Venuti, and (7) M. Pellerin; on which the three elements here mentioned, though, perhaps, not ſo accurately taken as thoſe tranſmitted you in the draught of my coin, are exhibited to our view. The legend or inſcription, however, on the reverſe has never, unleſs I am greatly deceived, been hitherto rightly explained.

The firſt of the characters preſerved by the medal I am conſidering is taken by Sig. Abate (8) Venuti for *Koph*, and I intirely agree with him in that notion. On my piece, however, it muſt be allowed ſomewhat different both from the correſpondent element given us by this celebrated antiquary and that which occurs on the ſimilar medals publiſhed by (9) M. Pellerin. It ſeems to reſemble, though not very ſtrongly, the letter *Kappa*, as it appears on certain antient Greek coins. I am nevertheleſs fully ſatisfied, from ſeveral conſiderations, that the cha-

(3) Paruta *Medagl.* &c.

(4) Laſtanoſa *Muſ. de las Medallas Deſconocid.* &c. En Hueſca, 1645.

(5) Maffe. *Veron. Illuſtrat.* Lib. III. c. vii. p. 259, &c.

(6) Ridolfin. Venut. ubi ſup.

(7) Peller. *Recueil de Medailles de Peuples & de Villes,* &c. Tom. III. p. 85, 86. A Paris, 1763.

(8) Ridolfin. Venut. ubi ſup.

(9) Peller. ubi ſup.

racter

racter in question must be a figure of *Koph*. The legend on the similar medals published by (10) Sig. Abate Venuti and M. Pellerin renders this indisputably clear. That character therefore will, I doubt not, be looked upon by those well versed in this branch of literature as a new form of the element *Koph*.

The second and third letters of our inscription, as they appear on the coins communicated to the learned world by Sig. Abate Venuti and M. Pellerin, are apparently the same, though on mine they are most certainly distinct characters; the first of them strongly resembling a form of the Punic or Phœnician *Lamed*, and the other being indubitably one of *Nun*. Admit this, and the word may be read KAVLIN, or CAVLIN; though the *Jod*, after the Punic and Phœnician manner, is here suppressed. Such a suppression amongst the Phœnicians and Carthaginians was by no means uncommon, as I have (11) elsewhere incontestably proved. This being allowed, we shall, perhaps, not find it so difficult to point out the place where all these medals were struck.

There is a small island in the Mediterranean only five miles from Malta, denominated antiently ΓΑΥΛΟΣ, or GAVLOS, both by the Greeks and the Romans; as we learn (12) from Diodorus Siculus, (13) Mela, and (14) Pliny. This island, which is

(10) Ridolfin. Venut. & Peller. ubi sup.
(11) *Philosoph. Transact.* Vol. LIII. p. 275.
(12) Diod. Sic. Lib. V.
(13) Pompon. Mel. *De Sit. Orb.* Lib. II. c. vii.
(14) Plin. *Nat. Hist.* Lib. III. c. viii. Lib. V. c. vii.

about thirty miles in circumference, was (15) occupied by the Phœnicians in very early times, and afterwards by the (16) Greeks. When the latter were possessed of it, the capital, named also GAVLOS, was one of those cities called by the Greeks ΑΥΤΟΝΟΜΟΙ, governed by it's own laws, and consequently, as it should seem, a kind of free independent state. This may be fairly inferred from several antient coins of that city, to be met with in the cabinets of the great and the curious, with the Greek word ΓΑΥΛΙΤΩΝ upon them. The Carthaginians therefore (17), who probably succeeded the Greeks in the occupation of this island, may reasonably be presumed likewise to have coined money in that capital, with a Punic inscription upon it. Nor can this well be denied, as medals of Cosyra, or Cossura, denominated by the moderns Pantallaria, adorned with such an inscription (18), sometimes occur; though that island, whatever figure it might have made when possessed by the Carthaginians, was considerably smaller than GAVLOS, known at present by the name of Gozo. The Phœnicians and Carthaginians seem to have assigned the latter, when masters of it, the appellation of קל, KAVL, or CAVL; which may, perhaps, be deemed equivalent to the (19) Hebrew קל, KAL, LIGHT,

(15) Diod. Sic. ubi sup. Phil. Cluver. Sicil. Antiqu. Lib. II. p. 444. Lugd. Batavor. 1619.

(16) Sil. Italic. Lib. XIV. ver. 274. Phil. Cluver. ubi sup. p. 445.

(17) Scyl. *Peripl.* Burchard. Niderfted. *Malt. Vet. & Nov.* p. 35. Helmestadii, 1660.

(18) *Saggi di Dissertazion. Accademich.* &c. Tom. I. p. 31.

(19) Val. Schind. *Lex. Pentaglot.* p. 1615. Hanoviæ, 1612.

SMALL,

SMALL, or the (20) Arabic أقل, AKAL, the comparative of قل, KAL, SMALLER; thofe nations confidering Gozo as a fmaller ifland, ftate, or diftrict, than Malta. As for the minute ifland of Hephæftia, or Lopadufa (21), though mentioned by feveral of the antients, it was looked upon as meriting little attention, or regard. The etymon laid down here feems more appofite and natural than that obtruded upon the learned world by Bochart.

If the Phœnicians and Carthagininians then called Gozo, when occupied by them, CAVL, an inhabitant of that ifland muft have been named by them CAVLI; which in the plural will give CAVLIM, or CAVLIN, according to the different ages in which the monuments exhibiting the word at firft appeared. That IN was fometimes a Punic plural termination, the BAALSAMEN of St. Auftin (23) feems clearly enough to imply; the word SAMEN being of the Chaldee dual form, and evincing the plural as well as the dual number in Punic mafculine nouns to have ended fometimes in IN. Several of the antient Maltefe, or Punic, numerals, publifhed by Girolamo

(20) Jacob. Gol. Lexic. Arabico-Latin. p. 1950, 1951. Lugduni Batavorum, 1653.

(21) Plin. *Nat. Hift.* Lib. V. c. vi. Ptol. *Geograph.* Lib. IV. c. iii. Athen. *Deipnofoph.* apud Bochart. *Chan* p. 554, 555. Father Froelich mentions a coin of this minute ifland, with the word ΛΟΠΑΔΟΥΣΣΑΙΩΝ upon it; from whence we may conclude, that the true antient name of it was ΛΟΠΑΔΟΥΣΣΑ, LOPADVSSA, the authority of medals being deemed inconteftable by the learned. Erafm. Froel. *Notit. Elementar. Numifm. Antiquor.* &c. p. 97.

(22) Bochart. *Chan.* Lib. I. c. xxvi. p. 554. Francofurti ad Mœnum, 1681.

(23) Auguft. *Locution.* Lib. VII. c. i.

(24) Me-

(24) Megiſerio and (25) Johannes Henricus Maius, not to mention the Punic (26) ABELONII of St. Auſtin, and other ſimilar inſtances, that might, with equal facility, be produced, manifeſtly prove the ſame thing. Nor ought it to appear ſtrange, that the Punic inſcription here ſhould be expreſſed in the nominative caſe; as a parallel inſtance occurs on the reverſe of a * (26) curious Punic coin of the iſland of Coſſura, now in the very valuable cabinet of the Reverend and learned Mr. Godwyn, Fellow of Balliol College, Oxford, which is perfectly well preſerved. As *Koph* therefore in (27) oriental names was not infrequently converted into *Gamma*, or G, both by the Latins and the Greeks, when ſuch names were adopted by thoſe nations; it can be no matter of ſurprize, that CAVL, by the aforeſaid converſion, and the addition of a Greek or Latin termination, ſhould become GAVLOS, the appellation aſſigned the iſle of Gozo by ſeveral antient writers. Hence we may conclude it more than probable, that

(24) Girolam. Megiſer. in *Deſcript. Melit.* c. i. f. 13. Lipſiæ, 1606.

(25) Johan. Henric. Maius in *Specim. Ling. Punic. in hodiern. Meliten. ſuperſt.* § 16. f. 482. apud Petr. Burman. in *Theſ. Antiq. &c.* T. XV.

(26) Auguſt. *de Hæreſib. &c.* c. lxxxvii. Boch. *Chan.* Lib. II. c. xvi. p. 851. Francofurti ad Mœn. 1681.

* (26) As ſome of the forms of the letters on this valuable coin differ from thoſe of the correſpondent elements in all the draughts of the ſimilar pieces that I have hitherto met with, it ſeems to me highly to merit the attention of the curious and the learned. See TAB. XI. n. 3.

(27) Bochart. ubi ſup. Lib. I. c. xlii. p. 737. c. xxvii. p. 569. & in *Phal.* Lib. I. c. iii. p. 20. Francof. ad Mœn. 1681.

the piece in queſtion was ſtruck in Gozo, though the precise time of that operation cannot now be ſo eaſily aſcertained.

From what has been ſaid, I flatter myſelf, the learned will admit the medal before me to have appeared firſt in the iſle of Gozo, and not in Malta, as ſome of the moſt celebrated antiquaries have ſuppoſed; eſpecially, ſince the people of the former iſland had a mint erected in their capital, from whence coins were emitted, with the word ΓΑΥΛΙΤΩΝ on the reverſe (28), when under the domination of the Greeks. One of thoſe coins has a place aſſigned it in my ſmall cabinet, and another in the valuable collection of the Reverend and learned Mr. Cracherode, Student of Chriſt-Church, Oxon. My piece in the main is well preſerved, though the three laſt letters of the legend on the reverſe have been effaced by the injuries of time; but Mr. Cracherode's, which is likewiſe in pretty good conſervation, has handed down to us that legend perfect and intire. A draught of my medal attends this paper (ſee TAB. XI. n. 2.), which for accuracy may be abſolutely depended upon. We may ſafely therefore, as I apprehend, attribute the Punic medal I am conſidering, as well as all others adorned with the ſame Punic characters, to the iſle, or city, of Gozo, to which it ſeems in reality to belong.

To what has been here advanced it may poſſibly be objected by ſome, that the iſland of Malta is much ſuperior, both in number of inhabitants and extent, to that of Gozo; and that moſt of the antient coins

(28) Peller. ubi ſup. p. 34, 35, 36.

ſimilar

ſimilar to that before me are brought from Malta, where they are not infrequently found. From whence they may be inclined to collect, as Sig. Abate Venuti (29) actually has done, that all ſuch Punic medals as that I am attempting to explain made their firſt appearance in the iſland of Malta. In anſwer to the firſt of theſe objections, I would beg leave to reply, that as the two iſlands here mentioned were ſo near one another, both of them free ſtates, coined their own money, were occupied by people of the ſame nation, and conſequently of the ſame religion; neither their extent nor the number of inhabitants they reſpectively contained can be of any great weight, with regard to the point in queſtion. But farther, the iſland of Gaulos, or Gozo, though leſſer than Malta, was ſo conſiderable, probably by reaſon of it's excellent ports, mentioned by (30) Diodorus Siculus, that Mela (31) and Pliny (32), in their ſhort liſt of the iſlands of the Sicilian ſea, towards the coaſt of Africa, place it even before Malta, the largeſt of them. And we learn from an antient inſcription, that it had the honour of being a municipium (33) in the Roman times. Nor will the ſecond objection be of any great force, when it is conſidered, that the two iſlands of Malta and Gozo were in (34) a

(29) Ridolfin. Venut. ubi ſup. p. 37.
(30) Diod. Sic. ubi ſup. Lib. V.
(31) Pompon. Mel. ubi ſup.
(32) Plin. ubi ſup. p. 164.
(33) Spon. *Miſc. Erudit. Antiq.* Chriſt. Cellar. *Notit. Orb. Antiq.* p. 655, 656. Cantabrigiæ, 1703.
(34) Joh. Quintin. in *Deſcript. Melit.* & Burch. Niderſted. ubi ſup. p. 35, 36.

manner

manner contiguous to one another; that the money of each was undoubtedly current, on account of their great vicinity, in both of them; and therefore that we may easily conceive a considerable number of pieces appertaining antiently to the latter to have been preserved, for many ages, in the former of those islands. And this is rendered still more probable by one or two inscriptions found in Malta (35), that originally belonged to the isle of Gozo. Nor is the cardinal point taken for granted in this objection, on which it almost intirely turns, by any means incontestable. For several of these medals, perhaps most of them, are brought into Europe from Tunis, as I take mine actually to have been, to which place they might as easily have found their way from Gozo as from Malta. All which being maturely weighed, the figures of the letters themselves, which ought to be the basis of all our reasoning relative to the word, formed of those letters, will, I would flatter myself, be allowed decisive in favour of my present opinion.

Sig. Abate (36) Venuti has, indeed, observed, that the God Mithra sometimes appears on such Punic coins as that considered by me here, in the same manner as on some of the antient Greek (37) medals of the Maltese; from whence he infers, that these Punic pieces are to be attributed to the island of Malta.

(35) Spon. *Misc. Antiq.* p. 190, 192. Jan. Gruter. *Corp. Inscript. ex Recens. & cum Annotat.* Joan. Geor. Grævii, p. 415. Amstelædami, 1707. Christoph. Cellar. *Notit. Orb. Antiq.* p. 655, 656. Cantabrigiæ, 1703.

(36) Ridolfin. Venut. ubi sup. p. 37.

(37) *Mem. de l' Acad. des Bell. Lett.* Tom. IX. p. 160.

Malta. But this inference, as I apprehend, is neither valid nor juſt. For as the inhabitants of Gozo were of the ſame religion with thoſe of Malta, as has been already remarked; 'tis natural to ſuppoſe, that the former, as well as the latter, might have impreſſed the effigies of the God Mithra, and any other religious, or rather ſuperſtitious, ſymbol, common to them both, on their coins.

I ſhall only beg leave at this time to add, that the MSS. from which ſome of the earlier editions (38) of Silius Italicus were printed, exhibited CAVLVM, as the true antient name of the iſle of Gozo. Phil. (39) Cluverius, Chriſtoph. (40) Cellarius, and Nic. (41) Heinſius, I know, look upon this lection as a corruption, and ſcruple not to pronounce it a depravation of the text. But their bare aſſertion, intirely unſupported, as it is, and ſtrongly oppoſed by a Punic medal of undoubted antiquity, by no means convinces me of the truth of what they aſſert; eſpecially, as there are other lections of the antient name of Gozo, two of which have for their initial letter K, or C. This

(38) Sil. Ital. Lib. XIV. ver. 274. Edit. Colin. Pariſiis, 1531. Sil. Ital. ubi ſup. cum Argumentis Hermanni Buſchii. Lugduni, 1598. Id. ibid. Ed. Plantin. 1600. Id. ibid. Lugduni, 1603. Id. ibid. Ed. Criſpin. in Corp. Vet. Poet. Latin. 1601. Id. ibid. cum Comment. Dauſqueii Sanctomarii. Pariſiis, 1618.

(39) Phil. Cluver. *Sicil. Antiq.* Lib. II. p. 444. Lugd. Batavor. 1619.

(40) Chriſtoph. Cellar. in Sil. Ital. Lib. XIV. ver. 274. Lipſiæ, 1695.

(41) Nic. Heinſ. in Sil. Ital. ubi ſup. Ed. Drakenb. Trajecti ad Rhenum, 1717.

we

we learn from (42) ſome of the manuſcripts, and the printed copies, of Strabo. The authority of theſe manuſcripts, in conjunction with that of thoſe of Silius Italicus abovementioned, ſeems to bring a freſh acceſſion of ſtrength to the preceding interpretation of the Punic word on my coin, and at the ſame time to receive itſelf no inconſiderable ſupport from that interpretation.

You will excuſe the trouble given on this occaſion, as ſome light may poſſibly be thrown on ſeveral very curious and valuable Punic medals, hitherto unexplained, by the paper now ſent you; and believe me to be, with all poſſible conſideration and regard,

Good Sir,

Your moſt obedient humble ſervant,

Chriſt-Church, Oxon.
Sept. 2, 1768.

John Swinton.

(42) Iſ. Caſaub. *Comment. et Caſtigat. ad Lib. Strabon. Geograph.* I. VI. VII. Gio. Pietro Franceſco Agius de Soldanis, *Dell' Origine Della Lingua Punica preſentamente uſata da Malteſi, Diſſertaz.* I. p. 50. In Roma, 1750.

P. 235, l. 13, 14, *for* n. 5. *inſert* n. 1. P. 249, l. 9. *for* n. 2, 3. *inſert* n. 1, 2. P. 253, l. 13, *for* n. 1. *inſert* n. 3.

Received September 29, 1768.

XXXVII. *Elucidation of an* Etruſcan *Coin of* Pæſtum, *in* Lucania, *emitted from the Mint there, about the Time of the Social War. In a Letter to* Charles Morton, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Cuſtos Archivorum of the Univerſity of* Oxford, *Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Dear Sir,

Read Nov. 24, 1768.

THE coin adorned with Etruſcan characters, of which you will meet with a deſcription (ſee TAB. XI. n. 4.) in this letter, at preſent in my ſmall cabinet, was ſome years ſince communicated to the learned world by Sig. Paſſeri; and aſcribed to the city (1) of Pæſtum, in Lucania, of which ſuch noble ruins are ſtill extant, by that ingenious author. This notion, upon farther examination, will be found by no means remote from truth; though, by attributing a wrong power, as I apprehend, to one

(1) Joan. Baptiſt. Paſſer. *De Num. Etruſc. Pæſtanor. Diſſertat.* in *Symbol. Litterar.* &c. Vol. ſecund. p. 13—35. Florentiæ, 1748.

 of

of the characters of the legend or inſcription on the reverſe, and conſequently aſſigning a falſe lection to that inſcription (2), Sig. Paſſeri ſeems greatly to have ſhaken, at leaſt, if not intirely overturned, his own opinion.

The head, with curled hair, on one ſide of this curious minute coin, may not improbably (3), as Sig. Paſſeri believes, be the effigies of ſome famous hero, or general, if not the founder of a city, that antiently bore a relation to the place where the piece was ſtruck; or it may poſſibly, (4) as the ſame learned gentleman alſo ſuggeſts, be allowed to point out to us ſome local deity. Two of the ſymbols on the reverſe undoubtedly repreſent a dolphin and an acroſtolium, though what that between theſe two was intended to point out to us, I cannot, with the ſame facility, take upon me to decide. The globule in the middle of this laſt, as it is termed by Sig. (5) Paſſeri, is moſt evidently on my medal ſuch a concha marina, or ſea ſhell, as we ſometimes meet with on antient coins. This perfectly well agrees with the two ſymbols abovementioned, and, in conjunction with them, clearly evinces the piece in queſtion to have been the produce of a mint erected in a maritime town.

It may not be improper to obſerve here, that a ſilver Greek medal, in fine conſervation, with the word KYMAION on the reverſe, in an oriental direction, from the right hand to the left, and two

(2) Id. ibid. p. 17.
(3) Id. ibid. p. 21.
(4) Joan. Bapt. Paſſer. ubi ſup.
(5) Id. ibid. p. 27, 28.

figures

figures extremely ſimilar to thoſe under the dolphin on my coin, which he takes to belong to the city of Cuma in Campania, has been publiſhed by (6) M. Pellerin.

With regard to the legend or inſcription, preſerved on the reverſe, it may not be improper to remark, that the fifth letter, taken (7) by Sig. Paſſeri for L, is, at leaſt in my opinion, moſt certainly V. This is ſufficiently apparent from even the four coins publiſhed by that ingenious gentleman, in the piece (8) here referred to; and ſtill more ſo from the medal now before me, as well as from another in the valuable collection of the Reverend and learned Mr. Cracherode, Student of Chriſt-Church, both of which are in the fineſt conſervation, and clearly exhibit V V, not LV. In other reſpects, the lection aſſigned this minute inſcription (9) by Sig. Paſſeri ſeems nearly to approach the truth, if it be not perfectly true. I ſhall therefore take the liberty to read it ZIVVTZI8, PHISTVVIS; which, according to the cacography, or uncouth manner of writing, of the Etruſcans (10), mentioned by me in a former paper, may, with ſufficient propriety, be deemed equivalent to the Latin PÆSTVM. The inſcription will, however, become quite another word, by the converſion of the firſt V into L, eſſentially different from the Latin name of the city wherein Sig. Paſſeri ſuppoſes it to

(6) Peller. *Recueil de Medailles de Peuples & de Villes, &c.* Tom. I. p. 47. pl. viii. n. 23. A Paris, 1763.
(7) Jo. Bapt. Paſſer. ubi ſup. p. 19.
(8) Id. ibid. p. 15—17.
(9) Id. ibid. p. 19, 20.
(10) *Philoſoph. Tranſact.* Vol. LI. Par. II. p. 858.

5 have

have been ſtruck; the letter L by no means, as a radical, entering into the compoſition of that name. Nor is the laſt element of the word (11) MVTIL, in any of the (12) moſt accurate draughts of Sig. Olivieri's Samnite-Etruſcan coins of Papius Mutilus, found to reſemble V, as Sig. Paſſeri has been pleaſed to aſſert. That element in all thoſe draughts, as well as on the reverſes of two medals of the ſame general in my ſmall collection (ſee TAB. XII. n. 2, 3.), perfectly well preſerved, is moſt apparenly L, in the true Etruſcan, or rather Samnite-Etruſcan, form.

If this notion, which to me appears inconteſtable, ſhould be admitted by the learned, they will of courſe reject the wild and arbitrary ſuppoſition of a modern Italian writer (13); who attributes the piece I am conſidering to Pliſtia, an obſcure inconſiderable town, mentioned (14) by Livy, and ſcarce ever, as far as I can recollect, by any other antient author. For the expunction of the L, a letter here purely ima-

(11) Sig. Gori has remarked, that one of the ſides of the Etruſcan V is now and then ſomewhat longer than the other. When this happens, part of the ſhorter ſide may be ſuppoſed to have been eraſed by the injuries of time; which, indeed, from the inſtances he has adduced, ſeems highly probable. Be that, however, as it will, even ſuch an incomplete V as this is very diſtinguiſhable from the Etruſcan L; as the principal part, or longer line, of the latter either always is or ought to be a perpendicular: whereas both the ſides of the former are in an oblique poſition, though not always equally ſo. Anton. Franciſc. Gor. *Muſ. Etruſc.* p. 414. Florentiæ, 1737.

(12) *Saggi di Diſſertaz. Accad. pub. lett. nella Nob. Accad. dell' antichiſſ. Cit. di Corton.* Tom. IV. p. 133. In Roma, 1743.

(13) Paſchal. Magnon. *De Ver. Poſidoniæ et Pæſti Originibus, &c.*

(14) Liv. Lib. IX. c. xiii. xiv.

ginary,

ginary, and the ſubſtitution of the v, in its ſtead, which the medal itſelf fully juſtifies, will not, as I apprehend, leave the leaſt room for ſo precarious, not to ſay abſurd, a ſuppoſition. Nor will many of the learned think this too harſh an appellation, as Pliſtia (15) ſeems to have been an obſcure inland town; whereas the ſymbols on the reverſe of my coin plainly evince it to have been ſtruck in a maritime city, and a city of very conſiderable note. It would therefore have been a little unlucky for a certain Engliſh writer (16) to determine in favour of the Italian author abovementioned, and aſſign this piece, as well as all others ſimilar to it, to Pliſtia, as he has done, had he been fully acquainted with the ſubject he wrote upon; but as this ſeems not to have been the caſe, his character, as an adept in this branch of literature, will not be greatly affected by the miſtake.

With regard to the antiquity of the medal I am now upon, I ſhall beg leave to remark, that the forms of the letters it exhibits, ſo perfectly ſimilar to thoſe of the elements preſerved by the coins of Papius Mutilus and Tiberius Veturius, heretofore explained, clearly indicate it to have been ſtruck about the time of the Social war. Beſides, that war is known to have raged in (17) Lucania; and the Lucanians are ſaid to (18) to have been one of the

(15) Phil. Cluver. *Ital. Antiq.* Lib. II. c. xv. p. 772. Lugd. Batavorum, 1634.

(16) See *The Ruins of Pæſtum*, p. 37. Lond. 1768.

(17) Appian. Alexandrin. *De Bell. Civil.* p. 375. See alſo *Philoſoph. Tranſact.* Vol. LII. Part I. p. 29, 30.

(18) Aut. Epit. Livian. LXXII. LXXIII.

principal

principal nations concerned at that time in the revolt from the Romans, if they did not actually take the lead in that revolt. They were commanded by M. Lamponius and Tiberius Cleptius, two able generals; the former of which defeated a body of Roman troops, under the command of Licinius Craſſus, formed the ſiege of Grumentum, in Lucania, and not a (19) little diſtinguiſhed himſelf, either in the firſt or ſecond campaign of the Social war. The Etruſcan, or rather Samnite-Etruſcan, characters on this Lucanian piece are therefore a very good proof of the truth of what is here advanced. For the principal confederated Italian ſtates, during the courſe of that war, uſed the Etruſcan characters, the antient letters of Italy, and that on their coins, out of deſpite to the (20) Romans. This has been already obſerved by the very ingenious Sig. Oliwieri, in the piece here referred to; nor will it, I would perſwade myſelf, be conteſted in any part of the learned world.

The celebrated Sig. Paſſeri ſuppoſes L to have (21) entered into the compoſition of the Etruſcan name of Pæſtum, by which he in a great meaſure, if not intirely, overturns his own hypotheſis, relative to the place where the coin before me firſt appeared. In order to account for this ſtrange ſuppoſition, he aſſerts the antients frequently to have placed the letter L after S; and (22) produces the words STLATVM,

(19) Appian. Alexandrin. ubi ſup.

(20) *Saggi di Diſſertazioni Accademiche pubblicamente lette nella Nobile Accademia Etruſca dell' antichiſſima Città di Cortona.* Tom. II. p. 53. In Roma, 1738.

(21) Jo. Bapt. Paſſer. ubi ſup. p. 19, 20, 21.

(22) Id. ibid. p. 20, 21.

STLITES, for LATVM, LITES, in support of his assertion. But here ST, not simply S, is added to L, the first radical letter, as an obsolete adjection, by no means essential to the word; whereas, in his Etruscan name of Pæstum (23), ST, not S, precedes L, by no means as an extrinsic addition, but as appertaining to the radix, and consequently as an essential part of the name. This therefore by no means comes up to the point. The proof is altogether as preposterous as the thing to be proved. But as I have already exceeded the limits I at first proposed to myself in this paper, it is time to conclude; which you will permit me to do, with assuring you that I am,

Dear Sir,

Your very faithful,

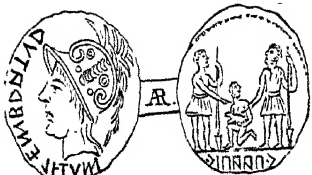
and most obedient humble servant,

Christ-Church, Oxon.
Sept. 5, 1768.

John Swinton.

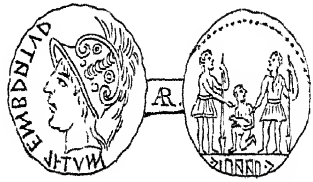
(23) Id. ibid. p. 19.

n. 1.



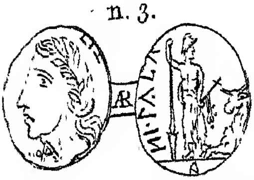
Penes Joannem Swinton, S.T.B. Oxonienf. R.S.S.

n. 2.



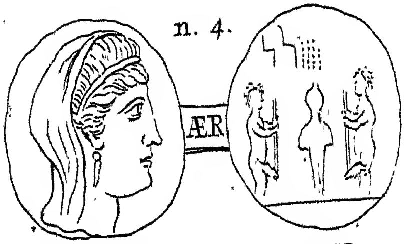
Penes Joannem Swinton, S.T.B. Oxonienf. R.S.S.

n. 3.



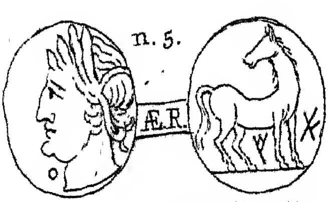
Penes Joannem Swinton, S.T.B. Oxonienf. R.S.S.

n. 4.



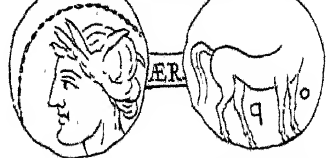
Penes Joannem Swinton, S.T.B. Oxonienf. R.S.S.

n. 5.



Apud Joannem Swinton, S.T.B. Oxonienf. R.S.S.

n. 6.



Apud Joannem Swinton, S.T.B. Oxonienf. R.S.S.

Received November 1, 1768.

XXXVIII. *Remarks upon a Denarius of the* Veturian *Family, with an* Etrufcan *Infcription on the Reverfe, never before publifhed. In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Cuftos Archivorum of the Univerfity of* Oxford, *Member of the Academy* degli Apatifti *at* Florence, *and of the* Etrufcan *Academy of* Cortona *in* Tufcany.

Good Sir,

Read Dec. 22, 1768.

I HAVE now in my fmall cabinet (fee TAB. XII. n. 1.) a Samnite-Etrufcan denarius, brought fome years fince by a gentleman of this Univerfity from Rome, refembling one of the Veturian family, by me formerly explained, in every particular but the legend or infcription, on the reverfe, and the letter in the exergue. As it will be needlefs to repeat what has already been faid, relative to this fpecies of coins, I fhall beg leave to refer, for a defcription of the medal before me, to one of (1) my

(1) *Philofoph. Tranfact.* Vol. LII. Par. I. p. 28—39.

former

former papers; and confine myſelf at preſent to the conſideration of the inſcription on the reverſe, which ſeems to merit the attention of the curious, and has not hitherto been communicated to the learned world. An attempt therefore to interpret this will not, I would flatter myſelf, prove unacceptable to the Royal Society; eſpecially, as ſome light may poſſibly be thereby thrown on ſeveral other ſimilar coins.

The inſcription now in view conſiſted originally, as I apprehend, of either five or ſix letters. If M. be taken for the prenomen here, the number of them amounted only to five; but if NI. be ſuppoſed to precede the name on this piece, ſix undoubtedly at firſt appeared. I was for ſome time diſpoſed to cloſe with the former of theſe notions, as M. is a prenomen that ſometimes occurs on the conſular coins; as the three perpendiculars forming ſo conſiderable a part of the Samnite-Etruſcan M are not always equidiſtant from one another, on ſuch medals as that before me; and as one of the tranſverſe lines of the Samnite-Etruſcan character repreſenting that element, on theſe coins, might have been eaſily effaced by the injuries of time. But examining the letters with greater attention, I afterwards found myſelf rather inclined to pronounce that part of the inſcription terminated by a point NI. though ſuch a prenomen has, perhaps, been ſcarce ever hitherto diſcovered on any antient monument. However, I will not preſume to determine abſolutely in favour of either of theſe points, though one of them muſt certainly be true. The former receives ſome countenance from two coins of Papius Mutilus, in my little collection, which exhibit the middle perpendicular of the Samnite-

nite-Etruſcan M as not equidiſtant from the other two; and the latter, in my opinion, appears at leaſt equally probable from the face of the inſcription. Accurate (2) draughts of both theſe medals of Papius Mutilus, one of which is in the fineſt conſervation, may be ſeen in the table referred to here. Of the two foregoing notions that will be judged the moſt agreeable to truth, which ſeems the moſt eligible to the learned.

The prenomen being thus diſpatched, I proceed to the name of the Italian commander preſerved on this coin. The firſt letter is apparently the Etruſcan L. The ſecond is as evidently V̇, adorned with the Samnite-Etruſcan accent, which (3) gave it the power of the Greek diphthong OΥ, or ov. This · here is only a point, equidiſtant from the two ſides; which confirms what I formerly ſuggeſted, in relation to (4) that accent. It is very viſible on the medal I am now endeavouring to throw ſome light upon. Part of the third letter has been defaced, but the remainder ſufficiently indicates it to have been the Samnite-Etruſcan *P*. Some faint traces of both ſides of the fourth element appear, which will probably be allowed to announce it v. The Samnite-Etruſcan accent in the former V̇, which conferred upon it the power of OΥ, or ov, and diſtinguiſhed it from the ſimple v, however, does not preſent itſelf to our view here. Thus ſtand the characters forming part of a name which, if I am not much miſtaken, will ſoon be more fully explained.

(2) *Philoſoph. Tranſact.* Vol. LVIII. Tab. XII. n. 2, 3. p. 255.
(3) *Philoſoph. Tranſact.* Vol. LII. Par. I. p. 32, 33.
(4) *Philoſoph. Tranſact.* Vol. LII. Par. I. p. 33.

As neither the Samnites nor the Etruſcans had in their alphabet o, they uſed the ſimple unaccented v for that element. This is evident from the celebrated tables of Gubbio, from the coins of Papius Mutilus, and from other Etruſcan and Samnite remains of antiquity. This we alſo learn from (5) Feſtus, (6) Quintilian, and other antient writers of good repute. I ſhall therefore not ſcruple to conſider the laſt letter of the inſcription I am now upon, ſuppoſing it v, as I verily believe it was, as equivalent to the Latin or Roman o, and conſequently ſhall read the four elements at firſt impreſſed upon the coin LVPO. This I would alſo look upon as part of the name LVPONIVS, though I queſtion whether or no that name has been ever yet obſerved on any other monument. Be that, however, as it will, the truth of my notion, from what immediately follows, will, I flatter myſelf, more clearly appear.

The Lucanian forces acted under the orders of M. Lamponius and Tiberius Cleptius, two generals of very conſiderable note, in the Social war. The former of theſe, according to (7) Appian, diſtinguiſhed himſelf by the defeat of a body of Roman troops, under the command of Licinius Craſſus, and the ſiege of Grumentum, in Lucania, either in the firſt or ſecond campaign of that war. As therefore, by ſuch a blow, he muſt have rendered no ſmall ſervice to the common cauſe; 'tis natural to ſuppoſe, that the allies did him the honour of impreſſing his name on ſome of their coins. And that this

(5) S. Pomp. Feſt. p. 339. Lutetiæ Pariſiorum, 1681.

(6) M. Fab. Quintilian. Lib. I. c. 4. Mar. Ver. Flac. apud Feſt. ubi ſup.

(7) Appian. Alexandrin. *De Bel. Civil.* p. 375.

this was really the caſe, we may fairly preſume, as Papius Mutilus (8) and Tiberius Veturius, two other Italian generals, his cotemporaries, were actually treated with the ſame mark of diſtinction, at the ſame time, for their laudable conduct in the Social war. This we learn (9) from ſeveral antient medals, adorned with the names of thoſe commanders, that have been heretofore explained. I would therefore convert the M. LAMPONIVS of the printed copies of Appian into M. or NI. LVPONIVS (10), the prenomen and name pointed out to us by my coin. I ſay M. or NI. LVPONIVS, becauſe it ſeems altogether immaterial whether we aſſume M. or NI. for the prenomen here; though the latter of theſe, I believe, has ſcarce ever hitherto occurred on any of either the Roman or Etruſcan remains of antiquity. Notwithſtanding which, I find myſelf inclined to prefer it to the other, as has been intimated above. And in farther ſupport of this preference it may be remarked, that NI. in a manuſcript might eaſily have been taken for M., ſo ſimilar are they to each other (eſpecially if the ſide of N next to I had been by any accident defaced), by a careleſs tranſcriber. The medal itſelf moſt certainly ſeems to determine in favour of this opinion, though what the complete prenomen repreſented by NI. really was, I muſt not at preſent take upon me abſolutely to decide.

(8) *Philoſoph. Tranſact.* Vol. LI. Par. II. p. 853—865. & Vol. LII. Par. I. p. 28—39.

(9) *Philoſoph. Tranſact.* ubi ſup.

(10) Perhaps the prenomen of this commander was Nigidius. Other generals of the confederated rebels were denominated Herius Aſinius, Marius Egnatius, &c. which may poſſibly ſeem to give a ſort of ſanction to ſuch an opinion. Aut. Epit. Livian. Vell. Paterc. Appian. &c.

From what has been ſaid it appears highly probable, that M. or NI. LVPONIVS ſtood originally in the text of Appian, or rather in that of Cornelius Siſenna (11), who gave a very minute and particular deſcription of the Social war, or of ſome other Latin author followed by the Greek hiſtorian. That writer therefore ſeems to be emended, in the inſtance before us, by my coin; which will perhaps be allowed to have brought to light the true name of an Italian general, who commanded a body of the confederated rebels in the Social war, that had been loſt for many ages. And this is rendered ſtill more probable by the Samnite-Etruſcan accent, which makes the firſt V̇ in that name equivalent to the Greek diphthong ΟΥ, or OV. For that diphthong and L form the firſt ſyllable of LVPVS, or ΛΟΥΠΟΣ, from whence the word LVPONIVS ſeems apparently to have been derived, as we learn from the text of (12) Appian. Nor can it be denied, that (13) LVPIVS and LVPONIVS are, by analogy, as naturally deducible from the Roman or Italian cognomen LVPVS as APRIVS and APRONIVS (14) are from APER: whereas LAM-

(11) Corn. Siſen. *Hiſt.* Lib. IV. apud Non. Marcel. in voc. CONVENIRE, cap. iv. col. 633. l. 48, 49, 50. Genevæ, 1622.

(12) *Appian.* Alexandrin. *De Bel. Civil.* Lib. I. p. 374. Amſtelodami, 1670.

(13) Lud. Ant. Murator. *Nov. Theſaur. Inſcript. &c.* p. 1264. Vid. etiam *Ind. Univerſ. Claſſ.* 17 p. 2302. Mediolani, 1742.

(14) The family name APRINIVS, which ſometimes occurs, ſeems to be altogether the ſame with APRONIVS; the poſition of I in the place of O, and O in the place of I, having not been uncommon amongſt the antients. Of this QVICVM for QVOCVM, DIE CRASTINI for DIE CRASTINO, AGNOTVS for AGNITVS, OLLI for ILLI, to omit many other inſtances that

PONIVS,

PONIVS, the name exhibited by the printed copies of the last mentioned author, seems to carry along with it the air of a depravation; since no Greek or Latin source, from whence we can, with sufficient propriety, suppose it to flow, presents itself to our view.

From the preceding observations, which, I flatter myself, will be deemed by no means remote from truth, it will follow, that the medal before me is to be attributed to the Veturian and Luponian families. But instances of this kind (15) pretty frequently occur. The names of Tiberius Veturius and C. Papius Mutilus are (16) exhibited by another Samnite-Etruscan denarius, of the same age with this, which I have formerly explained; and several Roman consular coins struck about the time of the Social war, and the following age, adorned with the names of two illustrious (17) persons of different families, are to be met with in the cabinets of the great and the curious at this very day.

If what has been here advanced should meet with the approbation of the learned, they will not read

might be adduced, are inconteftable proofs. In like manner LVPINIVS and LVPONIVS might, by analogy, have been deemed antiently the same name; the second syllable of the latter of which the Samnites and Etruscans, for want of O, undoubtedly wrote PV. Lud. Ant. Murat. *Nov. Thesaur. Vet. Inscript.* Tom. III. p. MCCC. n. 12. Mediolani, 1740. Joh. Nic. Func. *De Adolescent. Lat. Ling. Tract.* p. 324. Marburgi Cattorum, 1723.

(15) Vid. Vaill. Patin. Morell. & Sig. Haverc. in *Num. Fam. Rom.*

(16) *Philosoph. Transact.* Vol. LII. Par. I. p. 21—39.

(17) *Philosoph. Transact.* ubi sup. p. 31.

 the

the ſhort inſcription I have been conſidering NIGIDIVS LVPVS, which I was once inclined to do; as a perſon of that name is not, I believe, to be met with in the whole circle of antiquity. Such a lection as this would not have the ſanction of any antient writer, of any antient coin, or of any other antient monument; whereas my lection, or rather my interpretation of what I conceive to be the true lection, of this name, is ſupported by the authority of Appian, which in it's turn likewiſe receives no ſmall acceſſion of ſtrength from this interpretation.

I ſhall only beg leave to add, that this interpretation, if admitted by the learned, will alſo enable us to add the Luponian family, as a new one, to thoſe collected by Dr. Vaillant and M. Morell, whoſe names, together with ſome of the actions of their moſt illuſtrious members, have been handed down to us, through a long ſeries of ages, by antient coins; and that I am, with great truth,

S I R,

Your moſt obedient humble ſervant,

Chriſt-Church, Oxon.
Octob. 31, 1768.

John Swinton.

XXXIX. De-

Received Nov. 14, 1768.

XXXIX. *Deſcription of a* Punic *Coin appertaining to the Iſle of* Gozo, *hitherto attributed to that of* Malta, *by the Learned. In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Cuſtos Archivorum of the Univerſity of* Oxford, *Member of the Academy* degli Apatiſti *at* Florence, *and of the* Etruſcan *Academy of* Cortona *in* Tuſcany.

Good Sir,

Read Dec 22, 1768.

THE Punic medal before me (ſee TAB. XII. n. 4.), of which I ſend you a ſhort account in this paper, has been publiſhed (1) by F. Montfaucon, (2) the Marquis Scipio Maffei, and Sig. (3) Abate Venuti; but not by Paruta and Laſta-

(1) Montfauc. *Ant. Expl.* T. II. par. 2. p. 293.
(2) Maff. *Veron. Illuſtr.* Lib. III. c. vii. p. 259. In Verona, 1732.
(3) Ridolfino Venut. *Diſſertaz. ſopra alcun. Medagl. Malteſ. in Saggi di Diſſertaz. Accademich. pubblic. let. nella Nobil. Accadem. Etruſc. dell' antichiſ. Cit. di Cortona.* Tom. I. p. 36, 37. In Roma, 1735.

noſa,

nosa, as Sig. (4) Abate Venuti has been pleased to assert. On one side the head of a woman veiled presents itself to our view, and on the other three Egyptian figures, according to the Marquis Scipio Maffei. 'Tis observable that my medal, as well as that communicated to the learned world by the last mentioned author, exhibits a sort of wings fixed on the hips of the two exterior figures, though nothing like such wings is visible on the similar medal published by Sig. Abate Venuti. M. l'Abbé Barthelemy (5) may be supposed to have had an eye to this coin, when he informed us, "that the god Osiris appears with his attri-"butes on the medals which the Phœnicians struck "in the isle of Malta;" and to have considered the symbols on the reverse, whatever they were originally expressive of, as relative to the worship of Osiris which prevailed amongst the Phœnicians in that island. The Marquis Scipio Maffei seems to take the whole type to be Egyptian, and to point out to us some mode of the Egyptian superstition; but Sig. Abate Venuti will have the figure in the middle to be the god (6) Mithra, and the other two worshipers of that deity, each of them seeming to offer a patera to him. Which of these opinions is true, or whether any of them be so, I shall not at present take upon me to decide.

That this medal was at first adorned with a short Punic inscription on the reverse, formed of the letters *Kopb*, *Lamed*, and *Nun*, and consequently struck

(4) Venut. ubi sup. p. 36.

(5) *Mem. de Litter.* &c. Tom. XXXII. p. 737. A Paris, 1768.

(6) Venut. ubi sup. p. 37.

in

in the isle of GAVLOS, or Gozo, is plainly deducible from the (7) draughts of it published by the Marquis Scipio Maffei and Sig. Abate Venuti. The latter, however, of those draughts approaches nearer the original than the former, with regard to the inscription, though neither of them gives us a perfect representation of the letters the piece exhibits. Of those letters the last only, or *Nun*, has been preserved intire on my coin, and this is so faint that it is little more than barely visible. Part of the second is just perceptible, and seems to indicate the whole to have been *Lamed*, as that element appears on the coin of Gozo by me formerly described. The first letter is so totally defaced that not the faintest traces of it can be discerned. I must not forget to observe, that the form of the *Nun* here is perfectly similar to, or rather exactly the same with, that on the medal of Gozo I have lately explained, though somewhat different from the characters endued with the power of that element on all the draughts of the coins of Gozo that have hitherto appeared.

It may not be improper to remark, that the piece I am considering will bring a fresh accession of strength to what has been advanced in (8) one of my former papers, relative to this species of coins, as well as to the Punic or Phœnician name of the people antiently inhabiting the isle of Gozo. I shall forbear at present drawing any other conclusion from the medal before me, or rather from the very faint remains of the Punic characters it has handed down to us,

(7) Scip. Maff. & Ridolfin.-Venut. ubi sup.
(8) *Philosoph. Transact.* Vol. LVIII. TAB. XI. p. 235—245.

 which

which are the principal object of my attention here. What has been farther offered by F. Montfaucon, the Marquis Scipio Maffei, and Sig. Abate Venuti, both with respect to the veiled head (9) and the symbols on the reverse, will probably be deemed little better than vague conjectures, scarce meriting the attention of the learned.

You will consider this as a small appendix to the paper lately sent the Royal Society on (10) a Punic coin, which I attributed to the isle of Gozo; and believe me to be, with the highest regard,

Good Sir,

Your much obliged,

and most obedient,

humble servant,

Christ-Church, Oxon.
Nov. 10, 1768.

John Swinton.

(9) Montfauc. Maff. & Venut. ubi sup.
(10) *Philos. Transact.* Vol. LVIII. Tab. XI. p. 235—245.

XL. *Obser-*

Received November 14, 1768.

XL. *Observations on an inedited Coin, adorned with two* Punic *Characters on the Reverse. In a Letter to* Mathew Maty, *M. D. Sec. R. S. from the Rev.* John Swinton, *B. D. F. R. S. Custos Archivorum of the University of* Oxford, *Member of the Academy* degli Apatisti *at* Florence, *and of the* Etruscan *Academy of* Cortona *in* Tuscany.

Good Sir,

Read Dec. 24, 1768.

ABOUT three months since, a friend of mine (see TAB. XII. n. 5.) brought me a small brass medal, exhibiting on one side the head of a woman, decked with ears of corn; and on the other a horse standing still, and looking behind him, or towards his tail, one of the usual symbols on the reverses of such Punic coins. The medal is pretty well preserved, and adorned with two Punic characters; one of which is placed near the horse's breast, and the other under his belly. Neither of them seems to have suffered much, if at all, from the injuries of time.

These two Punic letters may, as I apprehend, be safely pronounced *Aleph* and *Koph*, and must be considered as forming the first part of the name of

ſome noted city, either in Sicily or Africa. The learned, at leaſt that part of them the moſt converſant in the branch of literature I am now upon, have frequently, if not generally, attributed ſuch pieces as that before me to the iſland of Sicily. But M. l'Abbé Barthelemy, who differs from all other antiquaries in many of his notions, ſeems to reprehend me for adopting ſuch a ſuppoſition; though he has himſelf aſcribed ſeveral Punic coins, embelliſhed with ſimilar characters, to Sicilian towns. But it matters not where ſuch pieces as this, with Punic characters upon them, firſt appeared, provided they were ſtruck in places either dependent upon or in alliance with the Carthaginians. And that they were ſtruck in places either dependent upon or in alliance with the Carthaginians, M. l'Abbé will not, I preſume, deny; if he ſhould, the ſymbols themſelves, in conjunction with the characters preſerved on theſe coins, would render this point ſufficiently clear.

With regard to the coin I am now conſidering, as I cannot meet with any antient noted city of Africa, that had a mint erected in it, and a name beginning with the letters this piece exhibits; I cannot prevail upon myſelf, at leaſt for the preſent, to attribute it to any town in that part of the world. I ſhould rather think it might have belonged to Agrigentum, a very celebrated antient city of Sicily, where money was coined, when that part of the iſland in which it was ſeated either appertained to the Carthaginians, was in alliance with that people, or had ſome commercial connections with them. The moſt antient part of Agrigentum was denominated

minated AKPA, or ACRA, as we learn from the authors cited by (1) Bochart; and that the same name was used by the Carthaginians, it is by no means unreasonable to suppose. Perhaps the later Greek name AKPAΓAΣ, or some other appellation similar to it, might also have been in vogue amongst them. In either of which cases, the Punic elements *Aleph*, *Koph*, would very well answer to the Greek letters *Alpha*, *Kappa*; as the latter of those letters is well known not seldom to have been equivalent to the (2) Phœnician, and consequently the Punic, *Koph*.

This seems still farther to appear from the draught of a medal of Agrigentum, published by (3) Paruta, with those two characters, and those two only, upon it. As the *Alpha* and *Kappa* there may, with great reason, be deemed equipollent to the two Punic elements on the coin here described; such an equipollence, or rather coincidence, of characters will be looked upon as an additional proof of the truth, or, at least, the probability, of the notion I would now recommend to the consideration of the learned world.

It must be here remarked, that under the chin of the female head a globule presents itself to our view; which may be considered as an uncial mark, denoting the weight or value of the piece. Such globules as this (4) not infrequently occur on the

(1) Boch. *Chan.* Lib. I. c. xxix. p. 610, 611. Francofurti ad Mœnum, 1681.

(2) *Philosoph. Transact.* Vol. LIV. p. *138, *139.

(3) Fil. Parut. *La Sicil. Num.* in *Num. di Gergent.* n. 9.

(4) Honor. Arigon. *Numism. quæd. cujuscunq; form. & metal.* &c. in *Antiq. Urb. & Populor. Numism. cum not. numeral.*

 Etruscan,

Etruscan, Greek, and Roman coins; but I remember not to have met with any author who has observed, that they appear sometimes on the Punic. I have another Carthaginian medal (see TAB. XII. n. 6.), with such a globule on the reverse; from whence we may conclude, that this uncial mark was used, on certain occasions, on both sides of the Punic coins. That the globule exhibited by the piece before me may be supposed an uncial mark, seems apparent, not only from the size of the medal itself, but likewise from the difficulty of accounting for it on any other supposition. If what is here advanced should meet with the approbation of the learned, it will be a farther proof that the coin was struck in Sicily; in which island, (5) and it's neighbourhood, many such antient pieces first appeared. That this, however, was really the case, I must by no means take upon me positively to affirm.

Tarvisii, 1741. Fil. Parut. ubi sup. pass. Anton. Francisc. Gor. *Mus. Etrusc.* p. 419—431. Tab. CXCVI. CXCVII. aliique scriptor. plur.

(5) Iidem ibid. Many antient pieces, struck in Sicily, Magna Græcia, Etruria, &c. are adorned either with one or more of these globules; which are, with great reason, taken for uncial marks by the learned. The coins of Agrigentum, in particular, frequently exhibit such marks. One of the medals of that city has been published, by Paruta, with a single globule upon it, extremely similar to that handed down to us by the Punic medal considered here. Some of the globules on the Sicilian pieces, published by Paruta, are exceeding small; and, in this respect, greatly resemble that preserved by the Punic coin I have been endeavouring to explain. Fil. Parut. *Ex Sicil. Num.* in *Num. di Gergent. di Drag. Fium.* & alib.

But

But to whatever town or country my medal originally appertained, it undoubtedly evinces the character I formerly took for *Koph* not to be *Aleph*, as some have supposed it to be, but in reality to be endued with the power of *Koph*; though it has been ranked amongst the (6) forms of *Aleph*, in my Siculo-Punic alphabet, by mistake. As both those elements are not only visible, but tolerably well preserved, and as it were placed in contrast, on this coin, the truth of the point in question will the more clearly appear. I shall only beg leave to add, that if the piece was struck at Agrigentum, as I am inclined to believe it was, it must have been of a pretty early date, as the Carthaginians seem to have had no particular connections with that city for at least a century before the destruction of their republick; and that I am, with great consideration and esteem,

Good Sir,

Your very faithful,

and most obedient,

humble servant,

Christ Church, Oxon.
Nov. 12, 1768.

John Swinton.

(6) *Philosoph. Transact.* Vol. LIV. Tab. xxiv. p. 409.

XLI.

XLI. *Introduction to the following Observations, made by Messieurs* Charles Mason *and* Jeremiah Dixon, *for determining the Length of a Degree of Latitude, in the Provinces of* Maryland *and* Pennsylvania, *in* North America; *by the Reverend* Nevil Maskelyne, *B. D. F. R. S. Astronomer Royal.*

Read Nov. 24, 1768.

MESSIEURS Charles Mason and Jeremiah Dixon, who observed the last transit of Venus over the sun, at the Cape of Good Hope, under the direction of the Royal Society, had been since engaged, by the Right Honourable Lord Baltimore and the Honourable Mr. Penn, to settle the limits between the provinces of Maryland and Pennsylvania, in North America; which they performed partly by trigonometrical, and partly by astronomical observations.

In the course of this work, they traced out and measured some lines lying in and near the meridian, and extended, in all, somewhat more than 100 miles; and, for this purpose, the country in these parts being all over-grown with trees, large openings were cut through the woods, in the direction of the lines, which formed the straightest and most regular, as well as extensive vistos that, perhaps, ever were made.

Messieurs

Meſſieurs Maſon and Dixon perceived that a moſt inviting opportunity was here given for determining the length of a degree of latitude, from the meaſure of near a degree and half. Moreover, one remarkable circumſtance very much favoured the undertaking, which was, that the country, through which the lines run, was, for the moſt part, as level as if it had been laid out by art.

The aſtronomical obſervations had been taken with an excellent ſector of ſix foot radius, conſtructed by Mr. Bird, the firſt which ever had the plumb-line paſſing over and biſecting a point at the centre of the inſtrument. This inſtrument was ſo exact, that they found they could trace out a parallel of latitude by it, without erring above 15 or 20 yards; in doing which, it ſhould be obſerved, that they generally uſed the ſame ſtars, commonly 6 or 8 or 10 in number, at the ſeveral ſtations, and made a double ſet of obſervations at each ſtation, with the limb of the ſector turned both to the eaſt and to the weſt. This ſector had been ſet up at the northermoſt point of the lines before-mentioned as proper for determining the length of a degree of latitude. In order to determine the difference of latitude between this point and the ſouthermoſt point of the lines, or the amplitude of an arch of a meridian contained between their parallels, it was neceſſary that the ſector ſhould be alſo ſet up at the ſouthernmoſt point, and the like obſervations repeated there, upon the ſame ſtars, which had already been obſerved at the northernmoſt point.

This plan of a meaſure of a degree in North America, Meſſieurs Maſon and Dixon ſubmitted to the

the confideration of the Council of the Royal Society, and offered to carry it into execution, at the expence of the Society, if they thought proper. The Council determined that fo ufeful and important a work fhould be completed, and accordingly fent out inftructions to Meffieurs Mafon and Dixon for the regulation of their operations; particularly requiring them to meafure the lines carefully over-again with fir-rods, which they fent to them, together with a brafs ftandard, of 5 foot, with which the rods were to be compared frequently, and the difference noted, and alfo the height of the thermometer at the time; for the lines had been all meafured before with a ftandard chain, which, though fufficient for the common purpofes of furveying, was by no means to be depended upon in fo nice an operation as that of meafuring a degree of latitude. The Honourable Mr. Penn was pleafed, at the requeft of the Royal Society, to grant the further ufe of his fector, before mentioned, and other inftruments, to the obfervers, for compleating this meafure.

The method purfued in this work, is, that which the level difpofition of the country pointed out. But the refult may be expected to be more accurate on this account, as meafures taken in a ftraight line, and on a level furface, are known to be capable of great exactnefs; and no adventitious errors are here introduced from any poffible errors of a chain of triangles. Meffieurs Mafon and Dixon having alfo determined the angle which the oblique line made with the meridian, by proper aftronomical obfervations, and the amplitude of the arch of their meridian line by feveral obfervations of zenith diftances of fixed ftars, made

made at both ends of the meridian, with the limb of the ſector turned both eaſt and weſt at each extremity; this meaſure of a degree ſeems to me to be as well ſtated, and as much to be depended on, as any that has been made; and will, I preſume, be thought a valuable addition to the other meaſures of degrees, which have been taken with great care and pains, by various learned men, particularly the members of the Royal Academy of Sciences at Paris, who have acquired ſo much juſt reputation by their valuable labours beſtowed on this ſubject.

It may not be improper to remark, that the level diſpoſition of the country this degree paſſes through, which, as I underſtand, alſo obtains further to the ſouth, and, in a great meaſure, to the north of the limits of the ſame, gives ſome advantage to this meaſure, with reſpect to the uſe that may be made of it in inquiring into the figure of the earth; as there is no room for ſuſpicion that the plumb-line of the ſector could be deflected materially from its proper poſition by the attraction of any mountain, or even elevated ground of a more moderate height, continued for a great length; which latter circumſtance, not taken notice of before, the learned Father Boſcovich has ſhewn, may produce a very conſiderable deviation of the plumb-line, in the elaborate treatiſe of the meaſure of a degree of the meridian between Rome and Rimini, taken by himſelf and his learned coadjutor, Father Le Maire.

XLII. *Observations for determining the Length of a Degree of Latitude in the Provinces of* Maryland *and* Pennsylvania, *in* North America, *by Messieurs* Charles Mason *and* Jeremiah Dixon.

Read Nov. 24, 1768.

IN this work, the first thing to be considered was, how to continue a right line: and this was done by setting up marks with the assistance of an equal altitude or transit instrument (for it was contrived so as to serve either purpose at pleasure), made by Mr. John Bird, of the same construction with that described by M. Le Monnier, in the preface to the single volume of the French Histoire Celeste.

The cylindrical ends of the cross axis of the telescope were laid in two angles of the supporters, which rose perpendicularly from a horizontal bar, that was fastened firmly to the upper part of the vertical axis. The axis of the telescope was set truly horizontal, by a spirit level hung on its cylindrical ends.

The brass frame, which receives the vertical axis, was screwed to a post fixed in the ground, in the direction of the line which was to be continued.

When the vertical wire in the telescope was brought to bisect any mark, it was kept in that direction, by confining firmly, between two pushing screws, a horizontal arm that projected from a collar that surrounded the vertical axis; and, to prove that a small shock would not alter its position, a small

preſſure was applied againſt one of the ſupporters, which being removed, it was carefully noted, whether the wire returned again to biſſect the mark.

At every ſtation (or mark) the teleſcope was turned two or three times after the mark was fixed in the line, to prove that the ſaid mark was truly ſet.—In general, the diſtances between the marks did not exceed a mile, nor were they leſs than half a one.

The teleſcope magnified about 25 times. Three or four marks were always left ſtanding, and on a little riſing ground they would all be ſeen in a right line, the vertical wire in the teleſcope biſſecting their centers without ſenſible error.

The marks made uſe of in continuing the lines were concentrical circles of black and white, painted upon both ſides of a board 14 inches ſquare. This board moved in mortices made in two poſts, which were drove into the ground; and, when the center of the ſaid mark was brought, by means of ſignals, into the line, it was faſtened by wedges to the poſts.

By means of a plummet, a peg was driven into the ground, and a notch cut in it, under the center of the ſaid mark, in order to ſecure the line.

In the evening, when we left off, a mark was placed before, and two or three left behind us; and in the morning the inſtrument was again ſet up in the ſame place, to prove that the marks were not

The tremour of the air (cauſed by the ſun's rays) was often very great; and, to avoid any error that might ariſe from the fluttering of the marks, we intermitted our operations ſometimes for five or ſix hours

 in

in a day, and were often obliged to make uſe of the morning or evening twilight.

In the continuation of the line, a perſon was left at the mark, behind the inſtrument, till another mark was ſet forward, to prove with a plummet that its center was not moved.

The viſto cut through the woods, in this work, was about eight or nine yards wide, and, in general, ſeen about two miles, beautifully terminating to the eye in a point.

The zenith diſtances of the ſtars, for determining the celeſtial arc, anſwering to the interval of the parallels of the northernmoſt and ſouthernmoſt points of the lines, were made with an excellent ſector of ſix foot radius, conſtructed by Mr. John Bird.

In the courſe of the work, for dividing the provinces of Maryland and Pennſylvania, the following lines were traced out, that offered themſelves for determining the length of a degree of latitude.

In the following fig. let N repreſent the northernmoſt point, and A the moſt ſouthern of the ſaid lines. Beginning at N, a meridian was traced from N to P. = $\overset{\text{mil.}}{14}\ \overset{\text{cha.}}{64}\ \overset{\text{lin.}}{8}$. In this line there were ſome hills, which were meaſured horizontally with a level, but the plains were meaſured with a chain.

P C = $\overset{\text{m.}}{2}\ \overset{\text{ch.}}{79}\ \overset{\text{lin.}}{27}$; C being in the parallel of latitude with P, which was determined by the ſector.

D C a meridian = $\overset{\text{m.}}{5}\ \overset{\text{ch.}}{2}\ \overset{\text{lin.}}{43}$, in which are three or four ſmall aſcents and deſcents.

The

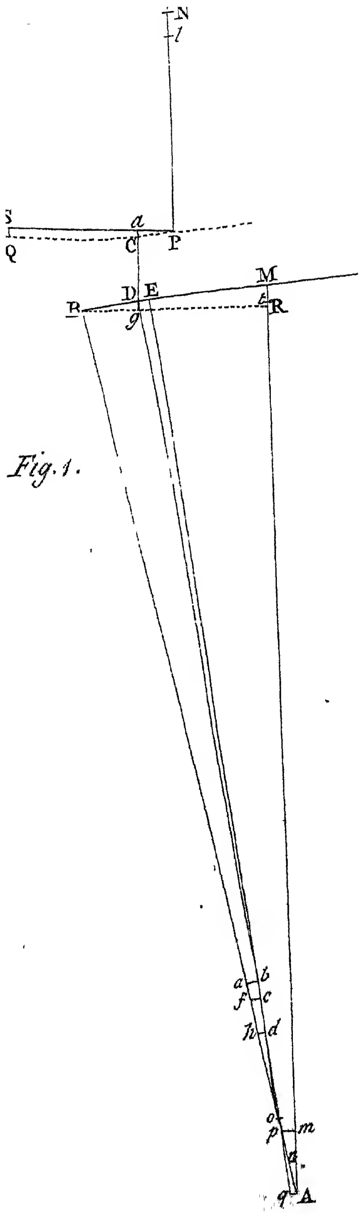


Fig. 1.

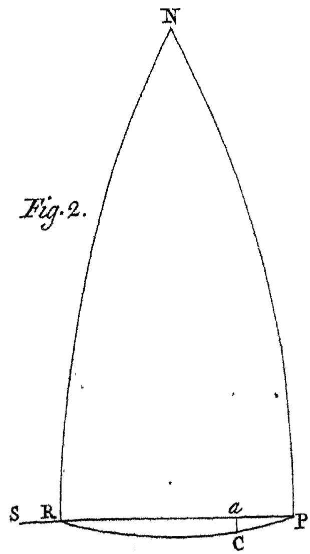


Fig. 2.

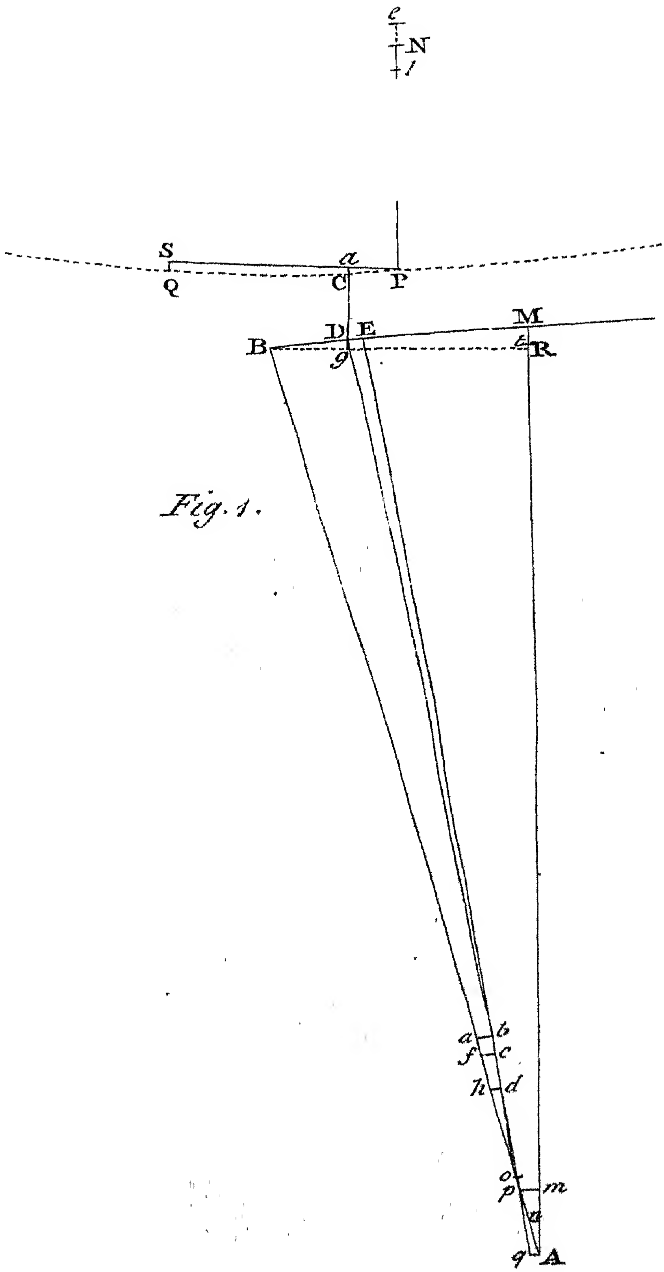


Fig. 1.

The points B D E and M are in a right line. BD = 22 cha. 51 lin., and the angle CDM = 86° $32\frac{1}{7}'$ nearly. Hence,

B is ſouth of D = 1 ch. 36 lin. = D *g*.

The line AB = 81 78 ch. 31 lin., in which is one gentle riſing hill, about half a mile over; all the reſt of the line is an entire level or plain.

Theſe meaſurements, expreſſed in Engliſh ſtatute miles and parts of the ſame, were made with a chain, eſtabliſhed from a braſs ſtatute yard, which was proved and corrected, in the courſe of the work, by another ſtatute chain (kept only for that purpoſe) made from the ſaid braſs yard. They were only deſigned for dividing the provinces of Maryland and Penſylvania: the ſame lines were re-meaſured afterwards with wooden rectangular levels, for the purpoſe of determining the length of a degree of latitude, as will appear in the ſequel of this work.

The point C was placed in the parallel of latitude of P, thus. Let N (ſee Tab. XIII. fig. 2.) repreſent the north pole of the terreſtrial globe; P and R two places lying in the ſame parallel of latitude RCP; PR an arch of a great circle = 10′ joining the ſaid points; and PN, RN two meridians. PN or the complement of the latitude of P being = 50° 16′ 42″, the angle NPR or the azimuth of the great circle PR was found by calculation to be 89° 55′ 51″. The going of the clock being found, by equal altitudes of ſtars, the times were computed when the ſame or other ſtars would paſs the azimuth of the line PR; and, at the time computed for any ſtar, the interſection of the croſs wires of the tranſit inſtrument being brought

to

to cover the ſtar, the teleſcope was turned down to the horizon, and a land-mark was fixed up at the diſtance of about half a mile, anſwering to the interſection of the wires. In like manner, by other ſtars, ſeveral other marks were fixed up, and the mean of all was taken. In this direction the line PR was continued; and though it was at firſt intended to extend it only to R, to the diſtance of 10′ of a great circle, it was in fact prolonged ſomewhat further, to S, PS being = 12,312 miles, or 10′ 45″ of a great circle. Now PC being = 2,991 miles, or 2′ 37″ of a great circle, the angle NPC is = 89° 58′ 55″; from whence NPS = 89° 55′ 51″ being ſubtracted, there remains the angle SPC or *a*PC = 3′ 4″, whence *a*C, or the diſtance of the parallel PCR at C, ſouth of *a*, ſhould be 14,1 feet. But it having been made a rule, in dividing the provinces of Pennſylvania and Maryland, to trace out the parallels of latitude by the obſervations taken with the aſtronomical ſector only, the ſector was put up at P and S ſucceſſively (ſee fig. 1.) and the zenith diſtances of the ſtars Capella α Lyræ, and others, were obſerved at both places; whence the point S was concluded to be 43 yards or 129 feet = SQ more northerly than P; and thence it was found by calculation, that the parallel of latitude PQ at the point C ſhould be 45½ feet, = *a*C diſtant from the great circle PS, and to the ſouth of the ſame; and the point C was placed accordingly, by laying off 45½ feet = *a*C, at right angles, to the line P*a* from the point *a* towards the ſouth.

*a*C found by the ſector, being 45½ feet, and found by the azimuth of the line PS being 14,1 feet only, it

it follows, that had the poſition of the point C been determined by the latter method, inſtead of the former, it would have been placed 31,4 feet more to the northward than it was found by the ſector; and, in conſequence, the length of the degree of latitude would have come out 21 feet longer. But the difference is ſo ſmall, that it only ſerves to confirm the exactneſs of the work, and renders it unneceſſary to enter into any conſideration, which of the two methods ought to be preferred.

The meridians NP, CD, and AM, were found by celeſtial obſervations. The method of proceeding was as follows:

To find the meridian AM, and the angle that the line AB makes with the ſaid meridian.

The equal altitude inſtrument being ſet up at the point A, with its vertical axis over the ſaid point, equal altitudes of ſtars were obſerved for finding the motion of the clock. The time was next computed when ſome northern ſtars would paſs the meridian by the clock, at which inſtant (ſhewn by the clock) the vertical wire in the teleſcope was brought to biſect the ſtar; and, the vertical axis of the inſtrument remaining fixed, the teleſcope was turned down in the ſame azimuth to the horizon, and a candle placed oppoſite to the vertical wire, as a point in the meridian.

And the time of ſtars paſſing an azimuth in the direction of the line AB, for determining the angle BAM, was found by bringing the vertical wire in the teleſcope to biſect a candle placed (about 1¼ mile from A) in the line AB; the teleſcope was then elevated to the ſtar, and the time when it paſſed the ſaid vertical wire taken.

The

The obſervations for determining the meridian A M, and the angle that the line A B makes with the ſaid meridian, were as follows :

1766. October.	Time per clock. h ′ ″	h ′ ″	Sum. h ′ ″	Half Sum. h ′ ″	
♄ . . . 11	19 51 27	21 17 10+	41 11 23+	20 35 41½+	equal altitudes α Cygni.
	52 49—	18 35+	11 24	35 42	
	54 13	19 56	11 23	35 41½	
			Mean ——	20 35 42	ſtar paſſed the meridian per clock.
				20 33 30	ſtar's apparent right aſcenſion.
				2 12	clock too faſt for ſidereal time.
☉ . . . 12	20 0 16	21 10 22::	. . . .	. . . .	equal altitudes α Cygni.
	1 42	11 47—	41 13 29—	20 36 44½	
	3 15	13 14	13 30	36 45	
			Mean ——	=20 36 45—	ſtar paſſed per clock.
				20 33 30	ſtar's apparent right aſcenſion.
				3 15—	= clock too faſt for ſidereal time.

22 20 13 α Urſa major } paſſed an azimuth in the direction of the line A B.
23 26 β Urſa major

1766. October.	Time per clock. h ′ ″	h ′ ″	Sum. ′ ″	Half Sum. h ′ ″	
☽ . . . 13	19 50 41	21 22 12+	15 33—	20 37 46½	equal altitudes α Cygni
	51 58	23 35½	15 33½	37 47—	
	53 20½	24 52	15 33	37 46½	
			ean :	20 37 46½	star passed per clock.
				20 33 30	star's apparent right ascension.
					Clock too fast. Hence the clock gained of sidereal ime the last 24^h, 62″.

22 21 16 α Ursa major } passed an azimuth in the d rection c the line A B.
22 24 21 β Ursa major }

	h ′ ″
The apparent right ascension of α Ursa major	22 49 8
The clock will be too fast, when this star will be on the meridian	4 22–
α Ursa will be on the meridian per clock	22 53 30—
In like manner I find that γ Ursa major will be on the meridian per clock . .	23 45 55½
And the pole ✱ at	0 50 27

At the instant when the clock shewed 22^h 53′ 30″, the vertical wire was brought to bissect the star α Ursa major; and then the vertical axis was made fast (the level shewing the horizontal position of the axis of the telescope and the line of collimation being just), the telescope was then brought down to the horizon, and by means of a candle seen through a small hole in a board, a mark, at the distance of 21 chai. 42 lin., was placed in a line with the said vertical wire. In

In like manner, when γ Urſæ majoris, and the Pole ſtar paſſed the meridian, as known by the clock, the vertical wire was brought, at thoſe inſtants of time, to the ſaid ſtars, and the teleſcope turned down to the horizon, in the ſame manner above mentioned, and the wire biſſected the candle, when held to the 1ſt mark, as near as could be judged.

1766. October.	Time per clock. h ′ ″	h ′ ″	Sum. h ′ ″	Half Sum. h ′ ″	
♂ . . . 14	20 2 21	21 12 15	41 17 35	20 38 47½	equal altitudes of α Cygni.
	3 48	13 47	17 35	38 47½	
	5 20+	15 14	17 35+	38 47½	
				20 38 47½	
				33 30	= ſtars A
				5 17½	Clock faſt.
	23 37 44½	0 20 49	0 3 36	0 1 48	equal altitudes of α Andromeda.
	40 6½	23 32	3 38½	1 49+	
	42 47	25 54	3 38	1 49+	
				0 1 49—	
				23 56 22	
				5 27—	clock too faſt.
				0 51 30	the Pole ∗ will be on the meridian.
				2 57 10½	β Urſa minor on ditto.

2 . 3 40 β Urſa minor paſſed the line A B.

Cloudy when the Pole ſtar paſſed the meridian.

At 2 57 10½ β Urſa minor was biſſected by the vertical wire for finding a meridian; and the teleſcope then turned down to the horizon, and by bringing a candle, at the diſtance of a mile, to be biſſec by the vertical wire, we there placed a mark. After theſe obſervations, the clock was wound up, in doing whic was ſtopped about 23″,

66. ber.	Time h ′ ″	clock, h ′ ″	Sum h ′ ″	H: f Sum. ″	
☿ . 15	19 52 14+	21 23 47	41 18 42½	20 39 2 +	ual ude Cygni.
	53 22½	25 10	18 42½	39 2 +	
	54 55½	20 28	18 42+	39 2	
				20 39 +	
				33 30	
				5 51+	ock fast.
	22 31 56	δ Ursa minot passed the line A B.			
	2 4 18	β Ursa minor passed ditto.			
♃ . . . 16	20 0 17	21 17 24	41 20 32	20 40 16	equal altitudes α Cygni.
	1 41	18 51½	20 32½	40 16+	
	3 8	20 16	20 33	40 16½	
				20 40 16+	
				20 33 30	
				6 46+	clock too fast.
	22 23 46	α Ursa major passed the line A B.			
	22 26 45½	β Ursa major. passed ditto.			
	22 32 45	δ Ursa minor passed ditto.			

1766. October.	Time per clock.		Sum.	Half Sum.	
	h ′ ″	h ′ ″	h ′ ″	h ′ ″	
21 . . 16	23 41 16	0 19 46	0 6 37	0 3 18½	equal altitude of α Andromeda.
	43 51½	22 46	6 37½	3 19—	
	46 51	25 22	6 38	3 19	
			Mean =	0 3 19	* passed the meridian per clock.
				23 56 22	*'s apparent A R.
				6 5	the clock too fast. Hence by the going of the clock, in the interval between α Cygni and α Andromed: passing the meridian.

	h ′ ″	
At	0 53 0	the Pole * will be on the meridian.
	2 58 41	β Ursa minor will be on ditto.
At	0 53 0	by the clock, the vertical wire was brought to the Pole * as usual; and, by means of a candle at the distance of a mile, a mark was placed, which fell, as near as could be judged, on the mark placed the 14th instant.
At	2 5 10	β Ursa minor passed the line A B.
	2 58 41	the wire was brought to β Ursa minor for finding a meridian as before; and, by means of a candle at the distance of a mile, a mark was placed, which fell 3 inches east of that placed the 14th.

N. B. In this last observation the axis of the escope was turned end for end; that is, the telescope itself was turned upside down. This proved the ends of the cylinder to be good.

8 . . . 17 In the evening, by means of a candle placed behind a board, with a ſmall hole in it over the mark placed the 13th inſtant, the line was extended to the marks at a mile diſtance, and there a mark placed, which fell $\frac{1}{4}$ of an inch eaſt of the mark placed the 14th inſtant.

From the whole, there are ſix obſervations, all within the ſpace of about 3 inches, at the diſtance of a mile: The mean was taken as a point in the meridian, north of the point A.

At this meridian point *m*. we laid off the line *mp*. at right angles to the meridian A, *m*, M, and, by a candle being placed at *o*, in the right line A B (about $1\frac{1}{4}$ miles from A), another candle was advanced along the line *mp*, till the vertical wire in the teleſcope biſſected both candles: Under the candle, at the interſection of *mp*, with A B, viz. at *p*, a mark was placed in the ground.

The ground between *m*, and *p*, being made ſmooth (it was level as a floor by nature) the diſtance *mp*, was meaſured twice, and found to be 5 chains, 14 feet, and $\frac{3}{10}$ of an inch.

With this ſame chain the diſtance *m* A was meaſured = 80 chains exactly.

For the Angle B A M, by celeſtial meaſure.

	h ′ ″		° ′ ″
The AR of the meridian when α Urſa major paſſed an azimuth in the direction of the line A B.	22 16 53, 8	by obſervations made October 12.	
	22 16 55, 1	D° on the . . . . 13.	
	22 16 54, 7	D° on the . . . . 16.	
Mean	= 22 16 54 5 =	. . . .	334 13 38
AR of α Urſa major		. . . .	162 16 54
Angle at the Pole = ...		. . . .	171 56 44

	h ′ ″		° ′ ″
AR's of the meridian when β Ursa minor passed the direction of the line A B.	1 58 7, 8	by observations made October 14.	
	1 58 14, 2	D° on the . . . . 15.	
	1 58 7, 1	D° on the . . . . 16.	
Mean of the first and last = ...	1 58 7, 5	=	29 31 52
A R of β Ursa minor			222 53 30
Angle of the Pole			166 38 22

Now having the distance of the ✱s from the Pole, the angles at the Pole, and the latitude of the point A, per spherics, we find the star's azimuth from the north = the angle B A M.

And for the said Angle by terrestrial measurement. As A M : Rad :: pm :

	° ′ ″
Tangent Angle p A m. = the Angle B A M. = ...	3 43 40
Angle per α Ursa major	3 43 25
D° per β Ursa minor	3 43 25
Mean	3 43 30 = Angle B A M.

In the same manner as the point m, in the meridian from A was found, points in the meridian, north of P and N, were also found; and having two points given, with them a right line was extended as follows; first N P. in a line with N e.——At N the equal altitude instrument was set up, and the vertical wire in the telescope was brought to bissect the mark at e; and there the vertical axis made fast. The spirit level shewing the axis of the telescope to be horizontal.

The vertical axis being well secured, the telescope part is taken off the supporters, and turned to point to the southward (carefully taking off, and putting it on the supporters so as not to move the axis); then on the farthest rising ground that could be seen, another mark was placed at l, in a right line with the vertical wire. A mark being left at N, the instrument is taken, and set up three or four feet south of the mark l, and having brought the vertical wire in the telescope in a right line with the marks at l, and N, the vertical axis is then made fast as before, the telescope immediately turned, and a third mark placed to the southward; and so the operation was continued.

In

In the ſame manner the lines P S, C D, A B, and A E were traced out; and, to prove that by this method a right line may be extended, we ſhall here give the reſult of continuing the lines A B and A E. A and D being two points between which a right line was to be drawn.

The point n was known to be nearly in the line A D. At A and n marks were placed, and 3 or 4 feet north of n (which was $\frac{1}{4}$ of a mile from A), the inſtrument was ſet up; then, in the ſame manner as above, the vertical wire in the teleſcope was brought in a line with A and n, and the vertical axis made faſt; the teleſcope was then turned to point to the northward, and a third mark placed, &c. &c. In this manner the line A n was continued to B.

Having continued the line to B, it fell $\overset{\text{chs.}}{22}\ \overset{\text{lin.}}{51}$, weſt of the point D: we then returned and laid off eaſtward off-ſets from the line A B, at every fifth mile from A, proportional to the diſtance from A, and at the end of every off-ſet placed a poſt, in order to form the line A D.

Off-ſets were meaſured very correct at $\left\{\begin{array}{l}\text{about } 10\frac{1}{2} \text{ miles} \ldots\ldots = ab. \\ \text{the } 10^{\text{th}} \text{ mile} \ldots\ldots = fc. \\ \text{about the } 9^{\text{th}}\ d^{\circ} \ldots\ldots = hd.\end{array}\right\}$ and at the 3 points, b, c, d, three marks were placed. At b the inſtrument was ſet up, to ſee if the 3 marks were placed in a right line; when it appeared they were not exactly ſo; but, on moving the middle one $\frac{1}{2}$ an inch eaſt, they then made a right line.

In this direction we continued the lines b, c, d, to A, which fell 2 feet 2 inches weſt of the point A, at q. The diſtance A q being ſo ſmall a quantity (gradually riſing) in ten miles, we thought it would be ſuperfluous to change the direction, and therefore returned to the point b, and extended the line northward, proceeding in the ſame manner as before.

Having continued the line to E, it fell 16 feet 9 inches eaſt of the point D.

Hence the off-ſets from this line to the true line A D, are as ſhewn by the Table B. And as we paſſed by the off-ſet poſts made from A B, we meaſured the diſtance of this line A E from the ſaid off-ſet poſts, which were as given in the Table D.

TABLE

Table B.

Miles from point A.	Off-sets in Feet	Inches	
0	2	2	to the eaſtward.
5	1	0	
10	0	2.2	to the weſtward, to give the true line A D.
15	1	4	
20	2	6	
25	3	8	
30	4	10	
35	5	11	
40	7	1	
45	8	3	
50	9	5	
55	10	6	
60	11	8	
65	12	10	
70	14	0	
75	15	2	
80	16	4	
82	16	9	

Table D.

Miles from point A.	Off-sets in Feet	Inches	
0	2	2	eaſtward
5	0	10	
10	0	0	he diſtance of the off-ſet poſts weſt from the line A E.
15	0	4	
20	4	8	
25	7	4	
30	8	3	
35	7	6	
40	8	5	
45	9	6	
50	10	1	
55	11	1	
60	10	6	
65	11	7	
70	12	1	
75	15	7	
80	16	7	
82	16	9	

From theſe Tables we ha
m the line A B, which
m the ſaid line A D, will

and A E; that is, the off-ſet poſts
the ine (A E) traced laſt, hat alſo
ows

Miles

Miles from point A.	Feet	Inches
0	0	0
5	0	2
10	0	2.2
15	1	0
20	2	2
25	3	8
30	3	5
35	1	7
40	1	4
45	1	3
50	1	6
55	0	7
60	1	2
65	1	3
70	1	1
75	0	5
80	0	3
82	0	0

This is a fufficient proof that the line A B is the arch of a great circle; but, as a farther confirmation that no error could arife, we obferved at different points in the line the right afcenfions of the meridian when the ftar δ Urfæ minoris paffed an azimuth correfponding to the direction of the line, being in the upper part of its circle.

The method of proceeding was in the fame manner, as defcribed before, for finding the angle B A M.

OBSERVATIONS for determining the right Afcenfions of the Meridian, when δ Urfæ Minoris paffed the Line A B.

June 25, 1764, we began at the point A, to trace the line A B; and the weather being fo cloudy prevented our making any one obfervation till July 10, though we attended every night. By this time we had continued the line 20 miles from A.

1764. July.	Time per watch. h ′ ″	h ′ ″	
10	15 49 0	17 5 10	equal altitudes of Antares.
	55 0	17 12 17	
	16 2 8	18 3	

	h ′ ″
Hence the * paffed the meridian per watch at	16 33 36
A R of Antares - - - - - - - - - - - -	16 15 1
Watch too faft for fidereal time - - - - - - - - -	0 18 35

		Time per watch.				
20 Mil from A.	1764, July.	h ′ ″	h ′ ″			
	♂ . . . 10	9 29 26	20 15 36	equal alt. α Aquilæ.	Hence ✳ paſſed at .	9 57 58
		34 18	21 40		Right aſc of α Aqui æ	9 39 18
		40 21	26 30			
					Watch oo faſt . . .	0 8 40

22 44 45 ♂ Urſæ minoris paſſed an azimuth in the di rection of the ine A B
18 44 watch too faſt when the ✳ paſſed the line.

22 26 the right aſcenſion of the me dian when ♂ U æ minoris paſſed the line A B.

		0 45	19 45 4	equal altitude α Lyræ.	Hence ✳ paſſed a .	8 59 0
		1 54	46 0		Right aſcenſion . . .	8 28 58
		2 56	47 1			
					Watch too faſt . .	0 30 2
43 Mi from 3		33 58	21 30 50	equal alt. α Cygni.	Hence ✳ paſſed at . . .	21 3 58
		35 33	32 23		Right aſcenſion . . .	20 33 25
		37 3	33 58			
					Watch too faſt . . .	0 30 33

22 57 36 ♂ U ſæ minoris paſſed the direct on of the line A B.
— 31 1 watch too faſt at this time.

22 26 35 = he ight a cenſion of the meridi n when ♂ U æ mino s paſſed he line A B

	1764. July.	Time per clock. h ′ ″	h ′ ″			h ′ ″
	♂ . . . 30	18 32 40	20 2 50	equal alt. of α Lyræ.	Hence ✱ paſſed at . . .	19 18 51
		33 42	. . . . . .		Right aſcenſion	18 28 58
		34 55	1 4 59		Watch too faſt	0 49 58
46 Miles from A.		20 56 12	21 49 13	equal alt. of α Cygni.	Hence paſſed at	21 24 23
		57 56	50 52		✱'s right aſcenſion . . .	20 33 25
		59 36	52 26		Watch too faſt	0 50 58

23 18 25 δ Urſæ minoris paſſed the direction of the line A B.
— 51 57 Watch too faſt.

22 26 28 = the right aſcenſion of the meridian when δ Urſæ minoris paſſed the line A B.

	August.					
	. . . 17	18 47 25	19 0 0	equal alt. of α Lyræ.	Hence ✱ paſſed at	19 12 42
		48 24	37 0		its right aſcenſion	18 28 58
		49 33	38 0		Watch too faſt	0 43 44
11 Miles from A.		20 42 45	21 50 48	equal alt. α Cygni.	Hence ✱ paſſed at	21 18 9
		44 8	52 12		its right aſcenſion	20 33 25
		45 30	53 30		Watch too faſt	0 44 44

23 12 48 δ Urſæ minoris paſſed the direction of the line A B.
— 45 39 = watch too faſt.

22 27 9 = the right aſcenſion of the meridian, when δ Urſæ minoris paſſed the direction of the line A B.

1764. August.	Time per watch. h ′ ″	h ′ ″		
☽ . . . 27	18 54 38	19 58 50	equal altitudes α Lyræ.	
	55 40	59 54		
	56 44	20 0 57		
Here we were 80 miles from A.	21 6 32	21 54 46	equal alt, α Cygni.	
	8 4	56 25		
	9 44	58 2		

	h ′ ″
Hence the * paſſed merid. per watch . . .	19 27 47
Right aſcenſion of α Lyræ	18 28 58
Watch too faſt	0 58 49

	h ′ ″
Hence paſſed at	21 22 31
*'s right aſcenſion	20 33 25
Watch too faſt	0 59 6

23 25 55 ♃ Urſæ minoris paſſed the direction of the line A B.
— 59 21 watch too faſt.

22 26 34 = the right aſcenſion of the meridian when ♃ Urſæ minoris paſſed the line A B.

Moſt of theſe equal altitudes were obſerved per Mr. Dixon, I judging the time by the watch, which had only the hour and minute hands; therefore the ſeconds muſt not be expected as from a good clock, nor does the problem require it, as the ſtar ♃ Urſæ minoris changed its azimuth very ſlowly.——The paſſage of the ſtar ♃ Urſæ minoris over the line A B was in general taken by myſelf.

The A R of meridian when ♃ Urſæ minoris paſſed A B. } 22 26 $0\frac{1}{2}$ / 22 25 $54\frac{1}{2}$ } by obſervations made on the 15th and 16th of October, 1766, at the point A.

Whether the ſmall effects of the aberration and nutation of the ſtar ♃ Urſæ minoris, at the different times, will add to or diminiſh from the correſpondence of theſe numbers, I have not determined; the above being ſufficient for the purpoſe intended; for if the direction of the line had been changed any quantity of note, it would have cauſed a much greater difference in the right aſcenſions of the meridians, when the ſtar paſſed the line, than any we here find.

The following are obſervations for determining the celeſtial arch between the points A and N. —— Thoſe marked with dots and ✳, thus, . . ✳ . . were made by Mr. Dixon.

1766. October. ☿ 8 We ſet up the ſector at the point A, in the middle of a weſt line, drawn between Cape Hinlopen and Cheſopeak Bay, and made the following obſervations.

N. B. Each revolution of the micrometer = 52″

PLANE OF THE SECTOR EAST.

✳'s names.	Neareſt point on the ſector.		Points on the micrometer.	Diff. between the points on the micrometer.		Apparent zen. diſtances.			
	°	′		°	″	°	′	″	
γ Andromedæ	2	45—	5 31— / 6 10	0	31,3	2	44	28,7	North.
β Perſei	1	35—	7 8 / 7 14	0	6,0	1	34	54,0	N.
δ D°	8	35—	7 32+ / 9 13+	1	25,0	8	33	35,0	N.
Capella	7	15+	6 16 / 4 39—	1	21,3	7	16	21,3	N.
β Aurigæ	6	25+	3 24— / 2 22¼	1	3,2	6	26	3,2	N.
Caſtor	6	5—	1 10 / 1 0½	0	9,5	6	4	50,5	South.

1766. October.			✱'s Names.	Nearest point on the sector.		Points on the micrometer.	Diff. between the points on the micrometer.		Apparent zen. distances.			
				°	′		°	′	°	′	″	
♃	9		Cloudy,									
♀	10		α Lyræ	0	5+	3 26½ 0 43½	2	19,0	0	7	19,0	North.
			γ Cygni	1	5—	4 42 6 6—	1	7,7	1	3	52,3	N.
			α D°	6	0—	5 41 5 46—	0	4,7	5	59	55,3	N.
			γ Andromedæ	2	45—	7 12 7 42	0	30,0	2	44	30,0	N.
		....✱....	β Persei	1	35—	8 15½ 8 22	0	6,5	1	34	53,5	N.
			δ D°	8	35—	5 51½ 7 33	1	25,5	8	33	34,5	N.
			Capella	7	15+	6 48½ 5 20½	1	20,0	7	16	20,0	N.
			β Aurigæ	6	25+	3 29½ 2 17½	1	4,0	6	26	4,0	N.
			Castor	6	5—	15 25 15 15½	0	9,5	6	4	50,5	N.
♄	11		α Lyræ	0	5+	8 26 5 41+	2	20,7	0	7	20,7	N.
			δ Cygni	6	5+	5 44— 3 37—	1	51,0	6	6	51,0	N.
			γ D°	1	5+	3 37 5 1	1	8,0	1	3	52,0	N.

		*'s Names.	Nearest point on the sector.		Points on the micrometer.	Diff. between the points on the micrometer.		Apparent zen. distances.			
1766. October.			°	′		°	′	°	′	″	
♄	11	α Cygni	6	0+	4 32 4 37½	0	5,5	6	0	5,5	North.
		γ Andromedæ	2	45—	6 14— 6 43	0	29,3	2	44	30,7	N.
		β Persei	1	35—	7 42— 7 46	0	4,3	1	34	55,7	N.
		δ D°	8	35—	5 38½ 7 17½	1	23,0	8	33	37,0	N.
		Capella	7	15+	8 51 7 23—	1	20,3	7	16	20,3	N.
		β Auriga	6	25+	7 20½ 6 8½	1	4,0	6	26	4,0	N.
		Castor	6	5—	4 31 4 20½	0	10,5	6	4	49,5	South.
☉	12	α Lyræ	0	5+	5 29— 2 43—	2	22,0	0	7	22,0	
		δ Cygni	6	5+	4 31½ 2 24½	1	51,0	6	6	51,0	
		γ D°	1	5—	6 26 7 43	1	9,0	1	3	51,0	
		α D°	6	0+	13 8 13 11½	0	3,5	6	0	3,5	
		γ Andromedæ	2	45—	2 37+ 3 18—	0	32,4	2	44	27,6	
... * ...		β Persei	1	35—	3 46 3 51	0	5,0	1	34	55,0	

		✱'s Names.	Nearest point on the sector.	Points on the micrometer.	Diff. between the points on the micrometer.	Apparent zen. distances.
1766. October.			° ′		° ′	° ′ ″
☉	12	δ Persei	8 35—	1 43 3 26	1 27,0	8 33 33,0
		Capella	7 15+	6 28 4 50	1 22,0	7 16 22,0
. . . ✱ . . .		β Aurigæ	6 25+	1 33+ 0 21+	1 4,0	6 26 4,0
		Castor	6 5—	15 37½ 15 29½	0 8,0	6 4 52,0

TURNED THE SECTOR PLANE WEST.

☽	13	γ Andromedæ	2 45—	6 2+ 5 29½	0 24,8	2 44 35,2
		β Persei	1 35+	5 43 5 44—	0 0,7	1 35 0,7
		δ D°	8 35—	6 2½ 4 24	1 22,5	8 33 37,5
		Capella	7 15+	7 4½ 8 40	1 27,5	7 16 27,5

	✱'s Names.	Nearest point on the sector.		Points on the micrometer.	Diff. between the points on the micrometer.		Apparent zen. distances.		
1766. October.		°	′		′	″	°	′	″
♂ 14	α Lyræ	0	5+	14 12 16 49	2	21,0	0	7	21,0
	α Cygni	6	0	13 16 13 16	0	0,0	6	0	0,0
	γ Andromedæ	2	45—	2 46 2 22	0	24,0	2	44	36,0
	β Persei	1	35+	2 5— 2 5+	0	0,6	1	35	0,6
	δ D°	8	35—	3 27+ 2 0—	1	19,6	8	33	40,4
	Capella	7	15+	1 50+ 3 36	1	29,7	7	16	29,7
	β Aurigæ	6	25+	2 51 4 18½	1	11,5	6	26	11,5
	Castor	6	5—	7 17 7 29	0	12,0	6	4	48,0
☿ 15	α Lyræ	0	5+	3 33 6 20—	2	22,7	2	7	22.7
	δ Cygni	6	5+	6 18 8 32	1	58,0	6	6	58,0
	γ D°	1	5—	7 4 5 43—	1	5,3	1	3	54,7
	α D°	6	0+	6 36+ 6 37½	0	1,2	6	0	1,2
	Andromedæ	2	45—	6 40½ 6 16	0	24,5	2	44	35,5
	β Persei	1	35+	5 48— 5 48—	0	0,0	1	35	0,0

1766. October.			Nearest point on the sector. °	'	Points on the micrometer	Diff. between the points on the micrometer. '	"	Apparent zen. distances. °	'	"
☿	15	δ Persei	8	35—	8 2 6 24	1	22,0	8	33	38 0
		Capella	7	15+	7 21½ 9 5	1	27,5	7	16	27.5
		β Aurigæ	6	25+	9 25— 10 44½	1	11,8	6	26	11,8
		Castor	6	5—	12 45+ 13 6	0	12,7	6	4	47 3
♃	16	α Lyræ	0	5+	4 48+ 7 35	2	22,7	0	7	22,7
		δ Cygni	6	5+	2 20½ 4 32—	1	55,2	6	6	55,2
		γ D°	1	5—	0 25 17 12½	1	4 5	1	3	55,5
		α D°	6	0+	0 8½ 0 9	0	0,5	6	0	0.5
		γ Andromedæ	2	45—	4 31½ 4 3½	0	28,0	2	44	32,0
. . . * . . .		β Persei	1	35+	3 25 3 25	0	0,0	1	35	0,0
		δ D°	8	35—	4 4 2 24½	1	23.5	8	33	36.5
		Capella	7	15+	1 20— 3 5	1	29,3	7	16	29,3
		β Aurigæ	6	25+	5 29½ 6 48	1	10 5	6	26	10,5
		Castor	6	5—	8 5 8 15¾	0	10,5	6	4	49,5

		✱'s Names.	Nearest point on the sector.		Points on the micrometer.	Diff. between the points on the micrometer.		Apparent zen. distances.		
			°	′		′	″	°	′	″
♀	17	α Lyræ	0	5+	5 18— / 8 5	2	23,3	0	7	23,3
		γ Cygni	1	5—	8 1½ / 6 41½	1	4,0	1	3	56,0
		α D°	6	0—	6 48½ / 6 48	0	0,5	5	59	59,5
		Capella	7	15+	8 43 / 10 26½	1	27,5	7	16	27,5
		β Aurigæ	6	25+	10 39— / 12 6	1	11,3	6	26	11,3
		Castor	6	5—	12 14— / 12 25+	0	11,6	6	4	48,4
♄	18	α Lyræ	0	5+	6 49 / 9 38+	2	25,3	0	7	25,3
. . . ✱ . . .		δ Cygni	6	5+	7 45— / 10 8	1	59,3	6	6	59,3
		γ D°	1	5—	5 15 / 4 3	1	4,0	1	3	56,0
		α D°	6	0+	16 47— / 16 49	0	2,3	6	0	2,3

THE RESULT OF THESE

Star's zenith distance at the point A.

PLANE OF THE

In the tent east.

		α Lyræ.		δ Cygni.		γ Cygni.		α Cygni.		γ Androm.
1766.	D.	° ′ ″	D.	° ′ ″	D.	° ′ ″	D.	° ′ ″	D.	° ′ ″
Oct.									8	2 44 28,7
	10	0 7 19,0			10	1 3 52,3			10	2 44 30,0
	11	0 7 20,7	11	6 6 51,0	11	1 3 52,0	11	6 0 5,5	11	2 44 30,7
	12	0 7 22,0	12	6 6 51,0	12	1 3 51,0	12	6 0 3,5	12	2 44 27,6
d h										
Mean 11 11		0 7 20,57		6 6 51,0		1 3 51,7		6 0 4,50		2 44 29,25
Aberration		— 17,11		— 18,40		— 17,33		— 17,75		— 4,08
Nutation --		+ 6,12		+ 4,14		+ 2,92		+ 2,25		— 7,53
Precess. Oct. 11, 1764.		0,0		0,0		0,0		0,0		0,0
Refract. ---		+ 0,12		+ 6,11		+ 1,06		+ 6,0		+ 2,75
Mean zen. dist. the 11th Oct.		0 7 9,70		6 6 42,85		1 3 38,42		5 59 55,0		2 44 20,39

PLANE OF THE

		α Lyræ.		δ Cygni.		γ Cygni.		α Cygni.		γ Androm.
October.									13	2 44 35,2
	14	0 7 21,0					14	6 0 0,0	14	44 36,0
	15	0 7 22.7	15	6 6 58,0	15	1 3 54,7	15	6 0 1,2	15	44 35,5
	16	0 7 22,7			16	1 3 55,5	16	6 0 0,5	16	-----
	17	0 7 23,3			17	1 3 56,0	17	5 59 59,5		
		------	18	6 6 59,3	18	1 3 56,0	18	6 0 2,3		
Mean ----		0 7 22,42		6 6 58,65		1 3 55,55		6 0 0,70		2 44 35,57
Aberration		— 16,74		— 18,31		— 17,40		— 17,93		— 4 79
Nutation --		+ 6,12		+ 4,14		+ 2,92		+ 2,25		— 7,53
Precess. ---		— 0,03		— 0,14		— 0,18		— 0,19		— 0,19
Refract. ---		+ 0,12		+ 6,11		+ 1,06		+ 6,0		+ 2,75
Mean zen. dist. Oct. 11, 1766.		0 7 11,89		6 6 50,45		1 3 41,95		5 59 50,83		2 44 25,81
D° plane east		0 7 9,70		6 6 42,85		1 3 38,42		5 59 55,0		2 44 20,39
Mean zen. dist. Oct. 11, 1766, at the point A.		0 7 10,79		6 6 46,65		1 3 40,18		5 59 52,92		2 44 23,10

OBSERVATIONS, AS FOLLOWS:

SECTOR EAST.

1766.	D.	β Persei.	D.	δ Persei.	D.	Capella.	D.	β Aurigæ.	D.	Castor.
		° ′ ″		° ′ ″		° ′ ″		° ′ ″		° ′ ″
Oct.	8	1 34 54,0	8	8 33 35,0	8	7 16 21,3	8	6 26 3,2	8	6 4 50,5
	10	1 34 53,5	10	8 33 34,5	10	7 16 20,0	10	6 26 4,0	10	6 4 50,5
	11	1 34 55,7			11	7 16 20,3	11	6 26 4,0	11	6 4 49,5
	12	1 34 55,0	12	8 33 33,0	12	7 16 22,0	12	6 26 4,0	12	6 4 52,0

β Persei.	δ Persei.	Capella.	β Aurigæ.	Castor.
1 34 54,55	8 33 34,17	7 16 20,90	6 26 3,80	6 4 50,62
− 1,09	+ 2,0	+ 5,48	+ 6,55	− 3,52
− 8,27	− 8,40	− 7,85	− 7,15	+ 4,70
0,0	0,0	0,0	0,0	0,0
+ 1,58	+ 8,55	+ 7,26	+ 6,43	+ 6,08
1 34 46,77	8 33 36,32	7 16 25,79	6 26 9,63	6 4 57,88

SECTOR WEST.

October.	β Persei.	D.	δ Persei.	D.	Capella.	D.	β Aurigæ.	D.	Castor.
13	1 35 0,7	13	8 33 37,5	13	7 16 27,5				
14	35 0,6	14	33 40,4	14	16 29,7	14	6 26 11,5	14	6 4 48,0
15	35 0,0	15	33 38,0	15	16 27,5	15	26 11,8	15	4 47,3
16	35 0,0	16	33 36,5	16	16 29,3	16	26 10,5	16	4 49,5
				17	16 27,5	17	26 11,3	17	4 48,4

β Persei.	δ Persei.	Capella.	β Aurigæ.	Castor.
1 35 0,33	8 33 38,10	7 16 28,30	6 26 11,28	6 4 48,30
− 1,79	+ 1,20	+ 4,98	+ 6,22	− 3,75
− 8,27	− 8,40	− 7,85	− 7,15	+ 4,70
− 0,16	− 0,14	− 0,07	− 0,02	− 0,10
+ 1,58	+ 8,55	+ 7,26	+ 6,43	+ 6,08
1 34 51,69	8 33 39,31	7 16 32,62	6 26 16,76	6 4 55,23
1 34 46,77	8 33 36,32	7 16 25,79	6 26 9,63	6 4 57,88
1 34 49,23	8 33 37,82	7 16 29,20	6 26 13,20	6 4 56,56

1766. December.

♃ 11 Set up the ſector at the point N, in the forks of the river Brandiwine, in Pennſylvania, and made the following obſervations.

		✱'s Names.	Neareſt point on the ſector.		Points on the micrometer.	Diff. between the points on the micrometer.		Apparent zen. diſtances.			
			°	′		′	″	°	′	″	
♄	13	γ Andromedæ	1	15+	5 20½ / 4 18—	0	54,8	1	15	54,8	North.
		β Perſei	0	5+	4 46+ / 3 18	1	20.3	0	6	20,3	N.
		δ D°	7	5+	4 8 / 4 6	0	2,0	7	5	2,0	N.
		Capella	5	50—	5 35 / 8 17	2	18,0	5	47	42.0	N.
☉	14	Cloudy.									
☽	15	γ Andromedæ	1	15+	7 32 / 6 29	0	55.0	1	15	55,0	N.
		β Perſei	0	5+	8 3— / 6 25	1	21.7	0	6	21,7	N.
...✱...		δ D°	7	5+	6 27 / 6 24	0	3.0	7	5	3,0	N.
		Capella	5	50—	4 46½ / 7 29½	2	19.0	5	47	41,0	N.
		β Aurigæ	4	55+	9 20½ / 6 29½	2	27.0	4	57	27,0	N.
		Caſtor	7	35—	9 14½ / 7 35	1	23.5	7	33	36,5	South.

	✱'s Names.	Nearest point on the sector. °	′	Points on the micrometer.	Diff. between the points on the micrometer. ′	″	Apparent zen. distances. °	′	″	
16	γ Andromedæ	1	15+	9 37— 8 33	0	55 7	1	15	55,7	North.
	β Perſei	0	5+	7 45½ 6 16½	1	21,0	0	6	21,0	
	δ D°	7	5+	5 21½ 5 19½	0	2,0	7	5	2,0	
	Capella	5	50—	7 11 9 46—	2	18,7	5	47	41,3	
	β Aurigæ	4	55+	9 39 6 47+	2	27,7	4	57	27 7	
	Caſtor		35—	6 34½ 5 5—	1	21,8	7	33	38,2	
17	α Lyræ . . .		20+	5 44— 7 42—	1	42,0	1	21	42,0	Sou
18	Cloudy.									
19	α Lyræ . . .		20+	5 1½ 7 1	1	43,5	1	21	43 5	
	β Perſei . . .		5+	8 42½ 7 14+	1	20,2	0	6	20.2	
	δ		5+	7 4 7 0	0	3,7	7	5	3 7	
	Capella . . .	5	50—	5 42½ 8 22½	2	16,0	5	47	44,0	
	β Aurigæ . .		55+	8 6+ 5 15	2	27,3	4	57	27,3	
	Caſtor		35—	4 45+ 3 16	1	21,3	7	33	38,7	

			*'s Names.	Nearest point on the sector.		Points on the micrometer.	Diff. between the points on the micrometer.		Apparent zenith distances.		
♄		20	Cloudy.	°	′		′	″	°	′	″
☉	..*..	21	α Lyræ	1	20+	3 2 5 0—	1	41,7	1	21	41,7

TURNED THE SECTOR PLANE WEST.

☉		21	β Persei	0	5+	6 4+ 7 42	1	29,7	0	6	29,7
			δ D°	7	5+	7 50— 8 8	0	10,3	7	5	10,3
			Capella	5	45+	4 5— 7 25½	2	56,8	5	47	56,8
				5	50—	4 5— 1 33½	2	7,2	5	47	52,3
			β Aurigæ	4	55+	7 0— 9 51	2	35,3	4	57	35·3
			Castor	7	35—	10 0+ 11 38+	1	30	7	33	30,0
☽		22	Cloudy.								
♂		23	D°								
☿		24	α Lyræ	1	20+	6 44— 5 0—	1	36,0	1	21	36,0
			γ Andromedæ	1	15+	4 41— 6 1½	1	4,8	1	16	4,8
			β Persei	0	5+	4 16½ 6 3—	1	30,2	0	6	30,2
	...*...		δ D°	7	5+	6 16½ 6 27	0	10,5	7	5	10,5

		✱'s Names.	Nearest point on the sector.		Points on the micrometer.	Diff. between the points on the micrometer.		Apparent zen. distances.		
1766. Decemb.			°	′		′	″	°	′	″
☿	24	Capella	5	50—	9 19— 6 47	2	7,7	5	47	52,3
. . . ✱ .		β Aurigæ	4	55+	6 2 9 1	2	35,0	4	57	35,0
		Castor	7	35—	6 31+ 8 16½	1	29,2	7	33	30,8
♃	25	Cloudy.								
♀	26	D°								
♄ . . ✱ . .	27	α Lyræ	1	20+	5 13+ 3 22	1	35,3	1	21	35,3
		γ Andromedæ	1	15+	8 19 9 31	1	4,0	1	16	4,0
		β Persei	0	5+	9 17½ 11 3	1	29,5	0	6	29,5
		δ	7	5+	12 16+ 12 26	0	9,7	7	5	9,7
		Capella	5	50—	6 32 4 7	2	9,0	5	47	51,0
☉	28	γ Andromedæ	1	15+	4 14— 5 26—	1	3,7	1	16	3,7
		β Persei	0	5+	2 42½ 4 27	1	28,5	0	6	28,5
		β Aurigæ	4	55+	8 14— 11 13+	2	35,6	4	57	35,6

PLANE OF THE

1766. December.		α Lyræ. ° ′ ″		γ Androm. ° ′ ″		β Persei. ° ′ ″
		......	13	1 15 54,8	13	0 6 20,3
		......	15	1 15 55,0	15	0 6 21,7
		......	16	1 15 55,7	16	0 6 21,0
	17	1 21 42,0		......		......
	19	1 21 43,5		......	19	0 6 20,2
	21	1 21 41,7		......		......
Mean		1 21 42,40		1 15 55,17		0 6 20,80
Aberration		+ 2,20		— 11,76		— 9,20
Deviation		— 5,66		— 7,60		— 8,19
Precess.		+ 0,48		— 3,12		— 2,64
Refraction		+ 1,36		+ 1,26		+ 0,10
Mean zen. dist. Oct. 11, 1766.		1 21 40,78		1 15 33,95		0 6 0,87

PLANE OF THE

1766. December.						
		......		......	21	0 6 29,7
	24	1 21 36,0	24	1 16 4,8	24	6 30,2
	27	21 35,3	27	16 4,0	27	6 29,5
		......	28	16 3,7	28	6 28,5
Mean		1 21 35,65		1 16 4,17		0 6 29,48
Aberration		+ 0,15		— 11,63		— 9,56
Deviation		— 5,66		— 7,60		— 8,19
Precess.		+ 0,53		— 3,76		— 3,05
Refraction		+ 1,36		+ 1,26		+ 0,10
Mean zen. dist. Oct. 11, 1766		1 21 32,03		1 15 42,44		0 6 8,78
D° Plane East		1 21 40,78		1 15 33,95		0 6 0,87
True mean zen. dist. 11 Oct. 1766, at the point N.		1 21 36,42		1 15 38,19		0 6 4,87
D° at the point A.		0 7 10,79		2 44 23,10		1 34 49,23
Difference		1 28 47,21		1 28 44,91		1 28 44,40
		28 44,91				
		44,40				
		43,98				
		45,09				
		44,34				

Mean = 1 28 44,99 = the true celestial arch between the points N and A.

SECTOR EAST.

	δ Persei.		Capella.		β Aurigæ.		Castor.
	° ′ ″		° ′ ″		° ′ ″		° ′ ″
13	7 5 2,0	13	5 47 42,0		. . . .		. . . .
15	5 3,0	15	47 41,0	15	4 57 27,0	15	7 33 36,5
16	5 2,0	16	47 41,3	16	57 27,7	16	7 33 38,2
	. . . .		. . . .		. . . .		. . . .
19	5 3,7	19	47 44,0	19	57 27,3	19	33 38,7
	. . . .		. . . .		. . . .		. . . .
	7 5 2,67		5 47 42,08		4 57 27,33		7 33 37,80
	— 8,61		— 3,20		— 0,60		— 3,78
	— 8,26		— 7,52		— 6,75		+ 4,20
	— 2,26		— 0,95		— 0,29		— 1,25
	+ 7,08		+ 5,80		+ 4,95		+ 7,55
	7 4 50,62		5 47 36,21		4 57 24,64		7 33 44,52

SECTOR WEST.

21	7 5 10,3	21	5 47 52,8	21	4 57 35,3	21	7 33 30,0
24	7 5 10,5	24	5 47 52,3	24	57 35,0	24	33 30,8
27	5 9,7	27	47 51,0		. . . .		. . . .
	. . . .		. . . .	28	57 35,6		. . . .
	7 5 10,17		5 47 52,03		4 57 35,30		7 33 30,40
	— 9,37		— 4,24		— 1,60		— 3,52
	— 8,26		— 7,52		— 6,75		+ 4,20
	— 2,56		— 1,09		— 0,32		— 1,36
	+ 7,08		+ 5,80		+ 4,95		+ 7,55
	7 4 57,06		5 47 44,98		4 57 31,58		7 33 37,27
	7 4 50,62		5 47 36,21		4 57 24,64		7 33 44,52
	7 4 53,84		5 47 40,60		4 57 28,11		7 33 40,90
	8 33 37,82		7 16 29,20		6 26 13,20		6 4 56,56
	1 28 43,98		1 28 48,60 this being a little wide of the rest is left out.		1 28 45,09		1 28 44,34

The following are Obſervations made at the Points N, and near P, in the Year 1764: But the Length of Time between theſe, and thoſe made at A, being near three Years—probably the Set made at N, in December 1766, may be beſt to be uſed in determining the Length of a Degree of Latitude.

STAR'S ZENITH DISTANCES.

SECTOR IN THE TENT, PLANE EAST.

1764.		γ Andromedæ.	β Perſei.	α Perſei.	δ Perſei.
		° ′ ″	° ′ ″		° ′ ″
January.	17	1 15 0,0	0 5 40,0	......	7 4 29,5
	19	....	0 5 39,6	......	7 4 31,0
	20	1 15 2,2	0 5 39,0	9 3 47,0	7 4 30,2
	21	1 15 1,3	0 5 37,5	9 3 49,7	7 4 30,8
	22	1 15 1,2	0 5 38,0	......	7 4 31,0
Mean, January	20	1 15 1,2	0 5 38,8	9 3 48,3	7 4 30,5
Aberration declin.		— 10,0	— 9,1	— 11,4	— 10,4
Deviation D°		— 3,3	— 5,7	— 6,2	— 6,7
Preceſs. fr. 1 Jan. 1764.		— 1,0	— 0,8	— 0,7	— 0,7
Refraction		+ 1,4	+ 0,1	+ 10,5	+ 8,3
Obſervatory S° of Tent		+ 0,3	+ 0,3	+ 0,3	+ 0,3
Mean zen. diſt. 1 Jan. 1764		1 14 48,6	0 5 23,6	9 3 40,8	7 4 21,3

SECTOR IN THE OBSERVATORY, PLANE WEST.

		γ Andromedæ.	β Perſei.	α Perſei.	δ Perſei.
January	26	1 15 4,7	0 5 41,7	......	7 4 32,8
♃	27	1 15 5,7	0 5 41,0	9 3 57,0	......
	28	1 15 4,5	0 5 44,0	9 3 57,5	7 4 33,0
	29	1 15 5,5	0 5 44,0	9 3 55,3	7 4 32,7
Mean, January	27½	1 15 5,1	0 5 42,7	9 3 56,6	7 4 32,8
Aberration in Declin.		— 9,0	— 8,6	— 11,1	— 10,3
Deviation D°		— 3,3	— 5,7	— 6,2	— 6,7
Preceſs. from 1 Jan. 1764		— 1,3	— 1,1	— 1,0	— 0,9
Refraction		+ 1,4	+ 0,1	+ 10,5	+ 8,3
Mean zen. diſt. 1 Jan. 1764		1 14 52,9	0 5 27,4	9 3 48,8	7 4 23,2
D° Plane Eaſt		1 14 48,6	0 5 23,6	9 3 40,8	7 4 21,3
True zen. diſt. at the point N, 1 Jan. 1764.		1 14 50,8	0 5 25,5	9 3 44,8	7 4 22,2
Preceſs. to Oct. 11, 1766		+ 49,45	40,56	.....	+ 34,61
Reduced to Oct. 11, 1766		1 15 40,25	0 6 6,06	.....	7 4 56,81

*'s Z. DIST. AT THE POINT N.

PLANE WEST.

		Capella		β Aurigæ.		Castor		α Lyræ.
		° ′ ″		° ′ ″			Jan.	° ′ ″
1764.	27	5 47 46,8	28	4 57 38,4		......	27	1 21 57,5
	28	5 47 47,0	29	4 57 36,4		......	28	1 21 57,0
January.	29	47 46,7	Feb. 2.	57 38,0		......		
February.	2	47 47,8		......	Feb.	......		1 21 57,3
	3	47 44,7		......	3	7 33 6,6	Ab.	— 9,5
		......	5	4 57 36,8	5	7 33 4,8	Dev.	— 9,4
		......	6	4 57 36,2	6	7 33 5,5	Prec.	+ 0,2
		......		......	8	33 6,8	Refr.	+ 1,5
		......						
		......					Feb.	1 21 40,1
		......						
		......					11	1 22 3,8
		......					13	1 22 1,3
Mean	30	5 47 46,6	1	4 57 37,2	5½	7 33 5,9	12	1 22 2,5
Aberration in Declin.		— 7,4		— 5,7		— 0,6		— 13,0
Deviation		— 8,8		— 9,2		+ 9,1		— 9,4
Precefs. from 1 Jan. 1764		— 0,4		— 0,1		— 0,7		+ 0,3
Refraction		+ 6,7		+ 5,8		+ 8,8		+ 1,5
Mean zen. dift. 1 Jan. 1764		5 47 36,7		4 57 28,0		7 33 22,5		1 21 41,9
								1 21 40,1
							Mean	1 21 41,0

PLANE EAST.

		Capella		β Aurigæ.		Castor		α Lyræ.
							Feb.	
February		......	Feb.	......		......	16	1 22 8,2
		......	18	4 57 35,7		......		......
	20	5 47 39,0	20	4 57 34,7	20	7 33 7,3	20	1 22 10,5
	21	5 47 38,7	21	57 35,0	21	7 33 6,3	21	1 22 8,8
	22	47 38,0	22	57 35,2	22	33 5,7	22	1 22 9,8
							26	1 22 10,8
Mean	21	5 47 38,6	21	4 57 35,2	21	7 33 6,4	22	1 22 9,6
Aberration		— 8,0		— 6,9		+ 0,5		— 14,8
Deviation		— 8,8		— 9,2		+ 9,1		— 9,4
Precefs. from 1 Jan.		— 0,7		— 0,2		— 0,95		+ 0,35
Refraction		+ 6,7		+ 5,8		+ 8,8		+ 1,5
Mean zen. dift. 1 Jan. 1764		5 47 27,8		4 57 24,7		7 33 23,8		1 21 47,3
D° Plane Weft		5 47 36,7		4 57 28,0		7 33 22,5		1 21 41,0
True zen. dift. at the point N, 1 Jan. 1764		5 47 32,3		4 57 26,3		7 33 23,1		1 21 44,2
Precefs. to Oct. 11, 1766		+ 14,56		+ 4,28		+ 18,97		— 7,03
		5 47 46,86		4 57 30,58		7 33 42,07		1 21 37,17

Stars Zenith Distances observed at a Point 7 chains 91 links North of P.

PLANE EAST.

1764. May.	Capella. ° ′ ″	May	α Lyræ. ° ′ ″	May	δ Cygni. ° ′ ″	May	γ Cygni. ° ′ ″	May	α Cygni. ° ′ ″
7	6 0 29,8	6	1 9 10,5	13	4 50 6,3	13	0 12 59,5	10	4 43 0,8
8	6 0 28,3	7	9 10,0	Ab.	+ 14,9		— 15,0	11	4 42 58,2
9	6 0 29,0	9	10,5	Dev.	+ 8,74		— 8,1	12	43 2,7
13	6 0 30,0	11	9,0	Prec.	— 3,05		+ 4,0	13	43 0,0
		12	10,0	Ref.	+ 5,6		+ 0,2	11½	
		13	7,7						
					4 50 32,5		0 12 40,6		
Mean of the 1st sett 10	6 0 29,3	10	1 9,6	May				mean	4 43 0,4
Aberration	— 1,5		— 12,3	19	4 50 8,0	19	0 13 0,0	Ab. +	16,5
Nutation	— 9,0		— 9,4	20	4 50 9,0	20	12 58,0	Dev. +	7,6
Precess. fr. 1 Jan. 1764	— 1,9		+ 1,1	23	50 11,3	23	12 57,0	Prec. —	4,5
Refract.	+ 7,0		+ 1,2	25	50 9,6	25	12 56,0	Refr. +	5,5
True zen. dist. 1 Jan. 1764.	6 0 23,9		1 8 50,2	22	4 50 9,5	22	0 12 57,75	mean	4 43 25,5
				Ab.	+ 13,2		— 13,5	May	
				Nut.	+ 8,74		— 8,1	19	4 43 1,0
				Prec.	— 3,25		+ 4,3	20	43 1,5
				Refr.	+ 5,6		+ 0,2	23	43 0,5
								25	43 2,3
				mean	4 50 33,8		0 12 40,6	22	
				Mean of the above.	4 4 32,5		0 12 40,6	mean	4 43 1,3
								Ab. +	14,9
True zen. dist. 1 Jan. 1764, from the mean of all the five observations.					4 50 33,15		0 12 40,60	Nut. +	7,6
								Prec. —	4,8
								Refr. +	5,5
								22 mean	4 43 24,5
								Mean of the above	25,4
								True zen. dist. 1 Jan. 1764, from the mean of all the observations	4 43 25,0

	May	α Lyræ.
	19	1 9 4,3
	23	9 3,8
	25	9 3,7
Mean of the 2d sett	22	1 9 4,3
Aberration		— 9,5
Nutation		— 9,4
Precess.		+ 1,1
Refraction		+ 1,2
Mean		1 8 47,7
Mean above		8 50,2
Mean of all the observat. 1 Jan. 1764		1 8 49,0

3

Star's Zenith Diſtances obſerved at a Point 7 chains 91 links North of P.

PLANE WEST.

1764.		Capella. ° ′ ″		α Lyræ. ° ′ ″		δ Cygni. ° ′ ″		γ Cygni. ° ′ ″		α Cygni. ° ′ ″
May ☉	28	6 0 32,7	27	1 08 59,0	27	4 50 14,8	26	0 12 52,6	26	4 43 4,3
June	1	0 32,3	28	8 59,5	28	4 50 51,0	27	12 51,0	27	43 4,2
	5	0 31,7	June		June					
			3	8 58,0	3	50 15,0	28	12 51,0	28	43 4,0
							June		June	
			4	8 59,8	4	50 15,3	3	12 51,0	3	43 7,5
			6	8 58,2	5	50 16,8	4	12 50,3	4	43 5,4
			7	8 57,3	6	50 16,3	5	12 49,8	5	43 6,5
			8	8 57,3	8	50 18,0	6	12 50,0	6	43 7,8
							7	12 49,0	7	43 8,0
							8	12 48,0	8	43 8,3
Mean	1	6 0 32,2	3	1 8 58,4	3	4 50 15,9	3	0 12 50,3	3	4 43 6,2
Aberration		+ 1,4		− 6,3		+ 10,4		− 11,3		+ 12,8
Nutation		− 9,0		− 9,4		+ 8,74		− 8,1		+ 7,6
Preceſs. from 1 Jan. 1764		− 2,2		+ 1,1		− 3,40		+ 4,6		− 5,2
Refraction		+ 7,0		+ 1,2		+ 5,6		+ 0,2		+ 5,5
Mean zen. diſt. 1 Jan. 1764 at a point 7 ch. 91 lin.		6 0 29,4		1 8 45,0		4 50 37,24		0 12 35,7		4 43 26,9
D°. Plane Eaſt		0 23,9		1 8 49,0		4 50 33,15		0 12 40,60		4 43 25,0
True zen. diſt. 1 Jan. 1764 at a point 7 cha. 91 lin. North of P		6 0 26,60		1 8 47,00		4 50 35,19		0 12 38,20		4 43 25,95
Preceſs. to 11 Oct. 1766		+ 14,56		− 7,03		+ 23,03		− 30,75		+ 34,59
True zen. diſt. 11 Oct. 1766		6 0 41,16		1 8 39,97		4 50 58,22		0 12 7,45		4 44 0,54
D°. at the point A		7 16 29,20		+ 7 10,79		6 6 46,65		1 3 40,18		5 59 52,92
Difference		1 15 48,04		1 15 50,76		1 15 48,43		1 15 47,63		1 15 52,38

1 15 48,04
15 50,76
48,43
47,63
52,38 } Arches by different Stars

Mean 1 15 49,45 = the celeſtial arch between the point 7 cha. 91 lin. N. of P and the point A.

Remarks on re-meaſuring the Lines with two rectangular Levels, or meaſuring Frames.

The levels uſed in this work were, each, 20 feet in length, and 4 feet in height. They were made of pine, an inch thick, and in form of a rectangle; the breadth of the bottom board was $7\frac{1}{2}$ inches, that of the top = 3 inches, of the ends = $4\frac{1}{2}$ inches, and the bottom and top were ſtrengthened with boards firmly fixed to them at right angles. The joints were ſecured with plates of iron, and the ends were plated with braſs. The plumb lines uſed in ſetting them level, were = 3 feet and 2 inches in length, and hung in the middle of the levels, being ſecured in a tube from the wind, in the manner of carpenters levels; wherefore we called theſe by the ſame name.

When the plumb-line biſected a point at the bottom, the ends were perpendicular.

Where the ground was not horizontal, or there were logs, &c. to paſs over, one end of the level was raiſed by a winch and pully.

The level being ſet, a ſhort ſtaff was drove into the ground (very near and oppoſite the plumb-line), in the top of which moved a thin plate of iron, about 12 inches long; at the ends of which were points, which were directed to the interſections of lines, drawn on the board that covered the plumb-line. By bringing the points in a line with one of the ſaid interſections, if the level was by accident moved, it might be diſcovered, and brought again to its place.

A level being thus marked, the end of the other was brought in contact with it, and marked in the ſame manner, before the firſt was moved; the firſt was then taken up, and ſet before the laſt. And ſo the operation was continued. Mr. Dixon attended one plumb-line and ſtaff, and I the other. The meaſure was carried on in a ſtrait line, and in the proper direction, by pointing the levels to the fartheſt part of the viſto that could be ſeen; this was readily and accurately done, on account of their lengths. The levels were frequently compared with the braſs ſtandard, of 5 feet, provided for that purpoſe, and the difference was noted between 8 times the braſs ſtandard, and the length of the two levels taken togerher; as may be ſeen in the 3d and 4th columns of the following table. This difference ſerves for reducing the meaſure taken with the levels, to what it would have been if it had been taken with the braſs ſtandard itſelf; ſee column 6th. For facilitating this compariſon of the levels with the braſs ſtandard, pieces of braſs were fixed into the bottom boards of the levels, on each of which was drawn a faint line. And one tenth of an inch at the end of the braſs ſtandard being divided into ten parts or hundredths of an inch, the difference between eight times the braſs ſtandard, and the two levels joined together, was with the help of a magnifying glaſs of a ſhort focus, determined to great accuracy. Moreover, the braſs ſtandard being liable to alter with the changes of heat and cold, a further correction becomes neceſſary on that account, in order

order to reduce the meaſures to the temperature of 62° of Fahrenheit's thermometer, which is the term to which the former operations of this kind have been reduced. For this purpoſe, the rate of expanſion of braſs is taken from Mr. Smeaton's experiments, made with a pyrometer of his invention (ſee Philoſ. Tranſ. Vol. XLVIII. Part II.) which is $\frac{232}{10000}$th of an inch upon a length of one foot for a variation of 180° of Fahrenheit's thermometer; whence the expanſion anſwering to four times the length of the braſs ſtandard, or 20 feet, or the length of one level, would be $\frac{464}{1000}$th of an inch for the ſame difference of the thermometer; and $\frac{258}{100000}$th of an inch for 1° of the ſame thermometer. Therefore, in order to find the correction of column 7th, the conſtant quantity ,00258 was multiplied by the difference of 62, and the degree of the height of the thermometer; and that product, again multiplied by the number of levels meaſured, gave the correction required in inches and decimal parts of an inch; which was additive or ſubtractive, according as the thermometer was higher or lower than 62 degrees.

In the following Proceſs, the

1 and 2 Columns contain the time of the day, M ſignifying morning, A afternoon.

3 - - - - the height of the thermometer at D°.

4 - - - - the quantity, in hundreth parts of an inch, that the two levels, taken together, were more or leſs than eight times the braſs ſtandard, or 40 feet.

5 - - - - the number of levels meaſured between the times, that the levels themſelves were meaſured with the braſs ſtandard.

6 - - - - the corrections, or quantity in inches, to be added to, or ſubtracted from, the number of levels meaſured each day, ariſing from the levels being more or leſs than the braſs ſtandard.

7 - - - - the correction, or quantity ariſing from the thermometer in inches.

Began, at the point N, to re-meaſure the lines with two rectangular levels, 20 feet each in length.

1768 February		1 h	2	3	4	5	6	7
♂	23	10	M	52	+,12	203	+12,18	+ 4,97
		3	A	53	+,12			
			The breadth of Brandiwine the 1ſt time			8,18	+ 0,49	— 0,47
			D° a 2d, where we croſſed			12,20	+ 0,73	

1768 February	1 h	2	3	4	5	6	7	
		Dº a third time			9,87	+ 0,39	− 0,20	
24	9	M	54	+,08				
	1½	A	44	+,12	325	+16,25	−13,41	
	5		39	+,32 : :				
25								
26	9½	M	40	+,12	247	+14,08	−12,42	
	4½	A	45	+,11				
27								Rain.
☉ 28								
29	8½	M	40	+,08	80	+ 3,20	− 0,41	Rain in the afternoon.
March 1	9½	M	32	+,23	320	+35,20	−20,64	
	4½	A	42	+,21				
2	8½	M	32	+,20	200	+15 10	−10,58	
	1	A	51	+,10	170	+ 9,09	− 5 48	
	5½		48	+,115				
3	8½	M	40	+,185	230	+17,82	−10,68	
	2	A	48	+,125	120	+ 8,10	− 4,33	
	5		48	+,145				
4	8½	M	31	+,19	200	+17,50	−14,19	
	2	A	38	+,16	150	+12,90	−10,83	
	5½		30	+,185				
5	8	M	27	+,18	220	+16,50	−15,89	
	1	A	41	+,12	120	+ 8 40	− 8 36	
	5½		29	+,165				
☉ 6								
7	8½	M	28	+,155	250	+15,37	−18,70	Very dry winds with frost.
	2	A	38	+,109	90	+ 4,41	− 5,80	
	5¾		36	+,105				
8	8	M	36	+,103	230	+ 3,8	−10,68	
	1½	A	52	−,037	170	− 1,02	− 5,92	
			45	+,06				

1768 March		1 h	2	3	4	5	6	7	
	9	8¼	M	51	+,065	270	— 3,10	— 2,44	Here subtract 4 inches, for going round the corner of a barn.
		2	A	66	—, 11	110	— 2,58	— 0,85	
		5½		52	+,015				
	10	8¾	M	58	—,01	179,2	— 1,79	— 1,15	
		0½	A	61	—,03				

This reached the point P.

Hence N P = 3914,45 levels + 199,02 inches — 178,40 inches = 78290,72 feet.

Began at the Point C.

♀	11	11	M	60	—,04	137,15	— 2.74	— 0,71	Rain.
♄	12	9	M	52	+,07	108	+ 2,16	— 1,95	
		1	A	58	+,01	156	+ 3,51	— 2,61	
		5½		53	+,08				
☉	13								
♃	17	Began where we left off, on the 12th instant, to measure as before.							
		10	M	34	+,09	132	+ 5,94	— 8,51	
		2	A	40	+,09	144	+ 6,12	— 8,91	
		5		36	+,08				
	18	8½	M	36	+,085	264	+ 7,65	—15,66	
		2	A	42	+,03	156	+ 4,06	— 9,45	
		5½		35	+,073				
	19	9	M	36	+,115	233,35	+14,93	—17,15	
			Noon	31	+,145				

This reached the point D.

Hence C D = 1330,50 levels + 41,63 inches — 64,95 inches = 26608,06 feet.

From D to g = 1 chain, and 36 links found before; = 89,76 feet.

☉	20	Sent to Philadelphia, about 40 miles distant, for tents, &c.						
♃	24	Began at the point B, to measure the line A B.						
		1½	M	49	—,08	132	— 5,28	— 4,42
♀	25	. . . .						
♄	26	Noon		57	—,05	211,05	+ 1,58	— 6,53
		6	A	43	+,065			
☉	27							

1768 March		1 h	2	3	4	5	6	7	
☽	28								Snow.
♂	29	8	M	40	+,19	264	+,18,48	—12,60	Compared two thermometers; they agreed within one division.
		2	A	47	+,09	264	+,13, 2	—12,60	
		5½		40	+,11				
☿	30	8½	M	38	+,14	288	+11,95	— 9,28	
		2½	A	61	+,025	192	+ 5,95	— 4,46	
		6¼		45	+,10				
♃	31	8¼	M	45	+,15	204	+12,85	— 4,21	By accident one of the thermom. was broke.
		2	A	62½	+,105	192	+13,15	— 2,97	
		6½		49	+,17				
April									
♀	1	8	M	39	+,195	324	+31,10	—13,79	
		2	A	52	+,19	204	+19.38	— 9,73	
		6		35	+,19				
♄	2	9	M	40	+,18	228	+18,24	—11,17	
		2½	A	46	+,14	168	+12,18	— 8,88	
		6¼		37	+,15				
☉	3								
☽	4	9½	M	38	+,04	312	+12,48	—14,08	Snow in the evening.
		3	A	51	+,12	192	+12,00	— 9,16	
		6		36	+,13				
♂	5								Snow.
☿	6	8½	M	37	+,14	228	+15,39	—10,58	
		3	A	51	+,13				
		6¼		38	+,13	204	+13,26	—10,70	
Crossed Bohemia river obliquely =						33,875	+ 2,21		
♃	7								Snow all day, and frost at night.
♀	8	11	M	37	+,23	396	+38,02	—24,01	
		6½	A	40	+,155				

1768 April		1 h	2	3	4	5	6	7	
♄	9	9	M	44	+,165	192	+ 7,96	− 5,69	
		2	A	57	00	204	+ 1,02	+ 4,21	
		6		51	+,02				
☉	10								
☽	11	9	M	59	−,017	288	−12,81	+ 0,37	
		2½	A	66	−,16	228	−14,82	+ 0,29	
		6½		59	−,10				
♂	12	8	M	47	−,02	348	−13,92	− 3,14	
		3	A	70	−,14	156	− 6,63	+ 1,21	
		6		60	−,03				
☿	13	9	M	64	−,08	240	−11,16	+ 4,95	
		1	A	76	−,105	156	− 6,86	+ 3,22	
		6		64	+,07				
♃	14	8¾	M	53	−,02	396	− 1,98	− 9,19	
		4½	A	53	,00				
♀	15	9	M	45	+,18	264	+23,76	−11,57	Rain in the af-ternoon: this day we pass through water two feet deep for half a mile
♄	16	8	M	46	+,20	396	+39,99	−11,23	
		4	A	56	+,205				
☉	17								
☽	18	8¼	M	52	+,11½	264	+ 6,20	− 1,70	
		Noon		67	−,02	192	− 1,44	+ 0,24	
		6½	A	58	−,01				
♂	19	8½	M	53	+,01	444	− 9,50	+ 2,23	
		3	A	74	−,075				
			after 3h aft.			132			
☿	20	8½	M	57	+,025	312	− 3,12	+ 4,43	
		2½	A	78	−,065	[illegible]	[illegible]	[illegible]	
		6½		61	−,02				

1768 April	1 h	2	3	4	5	6	7	
21	8½ 1½	M A aft. 1½ A	52 75	+,06 +,02	264 132	+ 7,92	+ 1,53	Left off in a swamp of water 18 inches deep.
22 23								Rain day and night.
24								Rain till 11h A M.
25 26								Swamps so full of water we could not proceed.
27	10 5½	M A	73 72	00 —,035	420	— 3,78	+11,38	
28	8½ 6	M A	54 61½	—,03 —,02	396	— 4,95	— 4,09	
29								
30	7 2 6	M A	60 76 73	+,19 +,11 +,11	336 108	+25,20 + 5,94	+ 5,20 + 3,47	
May 1								
2	8½ 6½	M A	54 56	+,05 +,09	384	+13,44	— 6,93	
3	10 3 6¼	M A	61 82 75½	+,115 +,02 +,075	288 108	+ 9,79 + 2,59	+ 7,06 + 4,74	This day we found the dist. between the 42d and 43d mile posts 3 levels and 6 feet more than usual between 2 mile posts.
4	10½	M	79	+,115	228	+13,18	+10,00	Thunderstorm all the morning passed the main branch of Choptank.

1768 May		1 h	2	3	4	5	6	7	
♃	5	8	M	70½	+,115	264	+ 8,97	+10,90	
		2	A	86	+,02	132	+ 3,63	+ 5,79	
		5½		72	+,09				
♀	6	9½	M	66	+,14	324			
		4	A	63	+,155				
		aft. 4h A				72	+29 11	+ 2,55	
♄	7	8	M	60	+,185	408	+ 25,5	+ 5,26	
		3	A	74	+,065				
☉	8								
☽	9	7½	M	63	+,03	528	— 8,69	+16,35	
		4½	A	85	—,095				
♂	10	8½	M	61	+,005	372	+ 1,49	+ 2,40	
		7	A	68	+,01				
☿	11	9½	M	70	+,111	528	+22,44	+12,26	Rain in the night and morning.
		6½	A	72	+,06				
♃	12	7	M	54½	+,23	264	+17.16	— 0,68	Rain last night.
		2	A	67	+,03	276	+ 3,03	+ 3,92	
		6½		68	+,015				
♀	13	8½	M	68	+,10	300			
		3	A	75	—,025		+ 7,33	+ 9,71	
		aft. 3h A				96			
♄	14	10½	M	66	+,145	264	+18,74	+ 5,45	
		3	A	74½	+,14				
☉	15								
☽	16	7½	M	57	+,19	384			
		3½	A	81	—,02		+22,44	+ 9,53	
		aft. 3½h A				144			
♂	17	8½	M	66	+,12	384			
		3	A	87	—,085				
		aft. 3h A				144	+ 4,75	+19,75	

1768 May		1 h	2	3	4	5	6	7	
☿	18	$8\frac{1}{2}$ 1	M A	67 90	+,085 +,14	144	+ 8,13	+ 6,13	Passed through Marshy Hope, water 4 or 5 feet deep.
♃	19	$9\frac{1}{2}$ 3	M A	69 86	+,15 +,075	264	+14,78	+10,55	
♀	20	8 3	M A aft. 3^h A	69 93	+,17 +,04	276 168	+23,31	+21,76	Great dews for 4 mornings past.
♄	21	9 4	M A	73 86	+,08 —,01	360	+ 6,30	+16,25	Great dew.
☉	22								
☽	23								Rain.
♂	24	7 3	M A aft. 3^h A	50 75	+,35 +,21	480 180	+92,40	+ 0,85	
☿	25	$9\frac{1}{2}$ 4	M A	59 56	+,28 +,34	396	+61,38	— 4,59	Rain last night and this morning. Passed through water part of the day.
♃	26	$9\frac{1}{2}$ $5\frac{1}{2}$	M A	58 53	+,38 +,36	528	+97,68	— 8,85	Rain last night and part this day, the levels continually wet.
♀	27	8 $3\frac{1}{4}$	M A aft. $3\frac{1}{4}^h$ A	65 79	+,18 +,14	396 264	+52,80	+17,02	Dry weather.
♄	28								
☉	29								
☽	30								

1768 May		1 h	2	3	4	5	6	7	
♂	31	$8\frac{1}{4}$ $3\frac{1}{2}$	M A	79 90	+,01 —,12	376	—10,34	+21,82	Very dry weather for 3 days past.

This reach'd to the North side of the river Nanticoke, near to the 7th mile post; here we left a mark.

♃	June 2	Passed over the river Nanticoke, and began at the 6th mile post from the point A.						
		8	M	84	+,075	300		
		$2\frac{1}{2}$	A	74	+,08			
		aft. $2\frac{1}{2}^{h}$ A				228	+20,59	+23,15
♀	3	9	M	76	+,155	396		
		3	A	85	+,02			
		aft. 3^{h} A				264	+29,04	+31,50
♄	4	$6\frac{1}{4}$ 1	M A	64 82	+,213 00	398,875	+21,15	+11,32 At the point A.

☉ ☽	5 6	Began at the 6th mile post, and measured Northward through the swamp of Nanticoke.						
		$7\frac{1}{2}$ 2	M A	67 77	+,12 +,055	230	+10,12	to the river 6,83
		Cross the river to the mark left the 31st of May.				34,67	+ 1,54	

This finished the line A B. Hence AB=21696,47 levels +892,34 inches +94,51 inches=434011,64 feet.

The breadth of the rivers was found by measuring a base, and taking angles with a Hadley's quadrant.

Note. The reckoning was kept by stretching a rope in the line to be measured (in general)=12 levels, which was often proved: and it was almost impossible that an error could arise; as we always began the rope with the same level, and ended it with the other; the rope not being removed till the last level was set.

The person that stretched the rope, sometimes Mr. Dixon, and sometimes myself, kept the account of the number of ropes measured: though the mile posts in the lines AB and DC were sufficient for that purpose, as the lines had been so often measured before.

In the line NP there were no mile posts, but two or three intermediate marks, which we found to agree in a general law with the levels.

Supposing the levels exactly =20 feet each; then in the line NP a mile per chain measure = a mile and 9,44 feet by the levels; and in the line CD a mile per chain measure=a mile and 9,86 feet by the levels.

In the line AB, what the levels make more than the chain, between the mile posts, as follows.

Miles from the point A.	Diff. in feet.
81, 80	14½
78	30½
76	25
75	14
72	43
70	32½
66	61
65	15−
63	33+
61	33½
60	16½
59	15½
58	16½
57	14½
56	15
55	13
54	12½
53	10½
52	12½
51	11
50	13
49	11
48	9½
47	9½
45	17½
44	7
43	9½

Miles from the point A.	Diff. in feet.
43, 42	74
40	16
39	8
38	14
37	14
36	14½
35	14
34	14½
33	14½
28	79½
27	15
25	32
24	20½
23	14
22	11½
21	12½
19	22
18	14½
15	29
13	14½
12	5
10	11½
9	8
8	7½
6	14
4	23
2	13
0	21½

Here appears to be an error of one chain = 66 feet, in the chain measure, as observed before, p. 318.

We took notice of these differences as we measured from B to A, always finding the miles greater by the chain measure, by the quantity above, which shews that the chain was continually extending itself by use; as we had direct proof of, being obliged to contract it every day, and re-adjust it to its proper length by means of the standard chain.

To

To find the Latitude of the Point N.

	δ Persei	Capella	β Aurigæ	Castor	α Lyræ.
	° ′ ″	° ′ ″	° ′ ″	° ′ ″	° ′ ″
True observed zenith dist. reduced to 1 Jan. 1764.	7 4 22,2	5 47 32,3	4 57 26,3	7 33 23,1	1 21 44,2
Stars decl. by Dr. Bradley.	47 0 40,0	45 43 53,0	44 53 44,2	32 22 56,8	38 34 34,0
Latitude by the different stars.	39 56 17,8	56 20,7	56 17,9	56 19,9	56 18,2

	39 56 17,8
	20,7
	17 9
	19,9
	18,2
Mean = ...	39 56 18,9 = the latitude of the point N.
Arch between N and A =	1 28 44,9
	38 27 34 = the latitude of the point A.
	39 11 56 = the mean latitude.

Cha. Mason.
Jere. Dixon.

The Length of a Degree of Latitude in the Province of Maryland and Pennsylvania, deduced from the foregoing Operations; by the Astronomer Royal.

THE difference of latitude of the points N and A, or the amplitude of the celestial arch, answering to the distance between the parallels of latitude passing through N and A, has been found by the sector, page 306, to be 1° 28′ 45″,0. The terrestrial measure of the distance of the said parallels is next to be found. This is composed of the sum of the lines N P, C D, D*g*, and A R, the last mentioned line being the reduction of A B to a meridian line passing through A: therefore B R expresses a parallel of latitude passing through B. Let B*t* be an arch of a great circle drawn perpendicular to the meridian line, A R produced. The triangle B A *t*, on account of the smallness of its sides with respect to the radius of the earth, and the smallness of the angle B A *t* =3° 43′ 30″ may be taken for a plane rectilinear triangle, in what follows, without any sensible error, as will appear to any one who makes the trial. Therefore it will be, by proportion, as radius is to the cosine of the angle B A *t*=3° 43′ 30″ so is A B=434011,6 English feet, to A *t*=433094,6 English feet. But this is to be lessened by the small quantity R *t*, or the distance of the parallel circle B R from the great circle

 B *t*,

t, which is to a third proportional to the diameter of the earth and the line B R, as c tangent of the latitude of the point B, to the radius. Whence $Rt=15,8$ feet hich subtracted from A t just found =

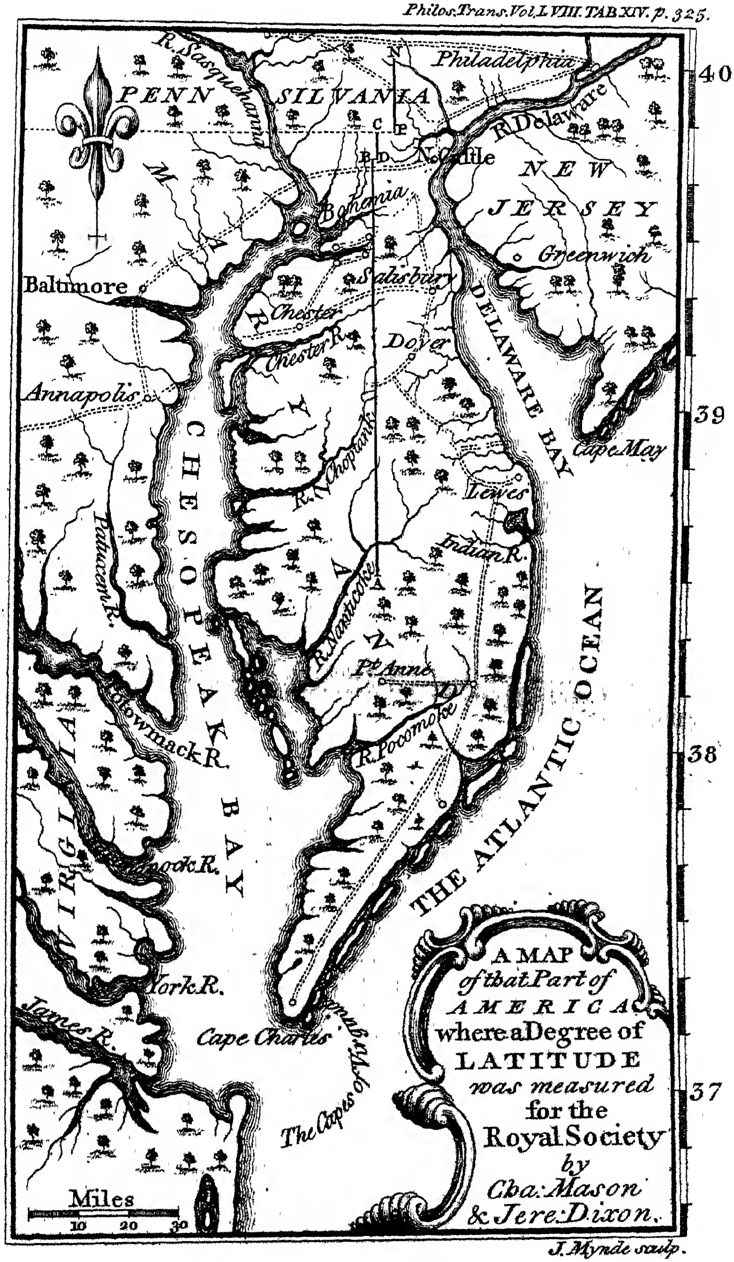
433094,6 leaves	AR=	433078 8 feet
To which add as found before	NP=	78290,7
	CD=	26608 0
	D g=	89.7
The sum is	=	538067 feet

= an arch of meridian intercepted between the parallels of latitude passing through the oints N and A, answering to the celestial arch 1° 28′ 45″.

Then say, as 1° 28′ 45″; is to 1° : : so is 538067 feet, to 363763 English feet, which is the length of a degree of latitude in the provinces of Pennsylvania and Maryand. The latitude of the Northernmost point N, was determined from the zenith istances of several stars, see page 323=39° 56′ 19″ and the latitude of the Southernost point A=38° 27′ 34″. Therefore the mean latitude expressed in degrees and inutes is =39° 12′.

To reduce this measure of a degree to the measure of the Paris toise, it must be remised, that the measure of the French foot was found upon a very accurate omparison, made by Mr. Graham, of the toise of the Royal Academy of Sciences at 'aris, with the Royal Society's brass standard, to be to the English foot, as 114 to 107. ee Philosophical Transact, Vol. XLII. p. 185. Therefore say as 114 : is to 107 : : so 363763 the measure of the degree in English feet, to 341427 the measure of the egree in French feet, which divided by 6, the number of feet in a toise, gives the ength of the degree=56904½ Paris toises, in the latitude 39° 12′ North.

Such is the length of a degree in this latitude, supposing the five feet brass standard nade use of in this measure to have been exactly adjusted to the length of the Royal ociety's brass standard. It was really adjusted by Mr. Bird, by his accurate brass scale f equal parts, which he makes such excellent use of in dividing astronomical instrunents, and which is just $\frac{1}{1000}$th part of an inch shorter than the Royal Society's brass tandard upon a length of three feet. If one would take notice of so small a difference, the ength of a degree just found must be lessened by $\frac{1}{36000}$th part, or by ten feet, in order o reduce it to the measure of the Royal Society's standard. Since I am treating of such iceties, may it be allowed me to add, that the five feet brass standard having been gain compared with Mr. Bird's scale, since its return from North America, appeared oth to myself and Mr. Bird to be just $\frac{1}{1000}$th part of an inch shorter than the scale, pon that side on which the hundredths of an inch are placed at one end, and $\frac{2}{1000}$ths f an inch shorter than the scale upon the opposite side? which diminution of its length s undoubtedly owing to the small wearing or battering which it has met with in the requent use that was made of it. But the divided side of the rod having been that which was made use of in measuring the levels, is what is to be regarded in the present ase. If one would allow for the wearing of the rod, one may suppose it to have suffered a gradual diminution; and then one must take a mean between its first length, which was the same with Mr. Bird's scale, and its present length, which is $\frac{1}{1000}$th of an inch er; as one may suppose it a medium to have been $\frac{1}{2000}$th part of an inch shorter than



J. Mynde sculp.

than Mr. Bird's ſcale; on which account the length of the degree ſhould be further diminiſhed by $\frac{1}{12000}$th part, or 3 feet, which added to 10 feet, the correction required on account of the difference of Mr. Bird's ſcale and the Royal Society's ſtandard, gives 13 feet to be ſubtracted from the length of the degree calculated above. The whole correction will perhaps be thought ſcarce deſerving of notice, eſpecially as an error of only 1″ in the celeſtial meaſure would produce an error of no leſs than 67 feet in the length of the degree. Moreover it is probable that the length of a degree has been already taken 10 or 20 feet too ſhort, by placing the point C too far to the Southward; which would about balance the ſmall correction in queſtion. Therefore, all things being conſidered, the length of the degree may be ſtated as given above, viz. =363763 Engliſh feet or 56904½ Paris toiſes. It muſt, however, be obſerved, that the accuracy of this reduction into Paris toiſes depends upon a ſuppoſition that the length of the French toiſe, which is of iron, was laid off by the gentlemen of the Royal Academy of Sciences, upon the braſs rod ſent over to them for that purpoſe by Mr. Graham (which was afterwards returned to him); in a room where the heat of the air anſwered to 62 of Fahrenheit's thermometer, or 15 of Reaumur's, or nearly ſo, which is probable enough, but is a point that does not appear to have been aſcertained. For, on account of the difference of expanſion of braſs and iron; 2 rods made of thoſe metals, however accurately they may be made of equal lengths at firſt, will only agree together afterwards in the ſame temperature of the air in which they were originally adjuſted together. It is fortunate that the uncertainty in the preſent caſe is but ſmall, ſince 20° difference of Fahrenheit's thermometer or 10° of Reaumur's produces, according to Mr. Smeaton's experiments, a difference of the expanſions of braſs and iron of only $\frac{1}{13500}$th part, which would cauſe an error of only 27 Engliſh feet or about 4 Paris toiſes in the length of the degree.

It is however to be wiſhed, that the proportion of lengths of the French and Engliſh meaſures might be again aſcertained by another careful experiment, in which the temperature of the air, as ſhewn by the thermometer, might be noted at the time.

[See the Map of the Country, where the foregoing Observations were made, TAB. XIV.]

POSTSCRIPT, BY THE ASTRONOMER ROYAL.

HAVING, ſome time ago, acquainted M. De la Lande, of the Royal Academy of Sciences at Paris, by letter, of this meaſure of a degree of latitude in North America, and at the ſame time expreſſed my doubts about the certainty of reducing it to French meaſure, from the proportion of the Engliſh to the French foot found by Mr. Graham; principally becauſe no notice had been taken of the height of the thermometer at Paris, when the length of the French iron toiſe was laid off upon the braſs rod ſent thither by Mr. Graham, whence the proportion of the two meaſures was afterwards determined by him; and having alſo mentioned my opinion of the expediency of making another experiment of the proportion of the two meaſures, in which every neceſſary circumſtance ſhould be noted; and that I might probably requeſt the favour of M. De la Lande to take the trouble to cauſe a French toiſe to be made for me, and to ſee it exactly adjuſted to their ſtandard, and then ſent to me; he has been pleaſed to lend me two toiſes,

toises, which he says are exactly adjusted to the standard of the toise used by Mess. De la Condamine and Bouguer in the measure of the degrees of latitude at Peru, in order to their being compared with the English measure. This comparison has been made by Mr. Bird, with his usual accuracy, while I was present, and also examined the same, since my account of the length of the degree of latitude aforegoing was printed; and the result is, that the longest of the two toises (for there is a small difference between them), and which has since been marked with the letter A, is equal to 76,738 inches by Mr. Bird's brass scale of equal parts, and the shortest toise, which is marked B, is $=$ 76,735 inches by the same scale; the height of Fahrenheit's thermometer in the room being 61 degrees. The mean of the lengths of the two toises is therefore $=$ 76,7365 inches by Mr. Bird's scale. But Mr. Bird's scale is $\frac{1}{1000}$th of an inch upon 3 feet shorter than the Royal Society's brass standard, and consequently $\frac{21}{10000}$th too short for the same upon 76,7365 inches; therefore $\frac{21}{10000}$th of an inch must be subtracted from 76,7365; which leaves 76,7344 for the length of the Paris toise in measures of the Royal Society's brass standard, in the temperature of 61° of Fahrenheit's thermometer. In the temperature of 62° it will be a little shorter; or it may be taken $=$ 76,734 inches in measures of the Royal Society's brass standard. This is $\frac{24}{10000}$th or about $\frac{1}{42}$d of an inch longer than was determined by Mr. Graham's experiment. Hence it appears, that I was mistaken in supposing, in p. 325, that the uncertainty about the true proportion of the English and French measures was but small, since the error in the former determination now appears to have been $\frac{1}{3197}$th of the whole, or equivalent to what might have been produced by a difference of 84° of Fahrenheit's thermometer. Whence it arose I cannot pretend to say, neither is it very material to enquire; but the fact is plain, and fully justifies the propriety of repeating the experiment.

I shall now state the length of the degree, measured by Messieurs Mason and Dixon, first in English feet, according to the Royal Society's standard, and then reduced to the French measure by the proportion just established.

From 363763 English feet, the length of the degree found by the 5 feet brass standard, see p. 324, I subtract 10 feet for the difference between Mr. Bird's scale and the Royal Society's standard, and 3 feet for the wearing of the brass rod; and there remain 363750 feet, according to the Royal Society's standard, for the length of the degree. But to this it seems proper to add 21 feet, in order to correct the position of the point C, determined by the sector, which cannot be so certain as that inferred from the azimuth of the line PS. See p. 270. Therefore the true length of the degree, according to the Royal Society's brass standard, in the temperature of 62° of Fahrenheit's thermometer, is 363771 feet, or 68,8960 English statute miles. To reduce this to the measure of the Paris toise, by the proportion above established, say as 76,734 is to 72, so is 363771 to 341328 French feet, or 56888 Paris toises, of the standard of that used [illegible] Peru.

[illegible] made use of by Mr. Bird, in finding the length of the toises by his scale, [illegible] which may [illegible] for the like purpose [illegible] [illegible] and longer [illegible] brought upon [illegible] was made to [illegible] and the other end [illegible]

at the same time touching the other cheek, the moveable cheek was screwed fast; and thus the toise was exactly contained between the cheeks without any shake, and it is evident that the interval between the cheeks was exactly equal to the length of the toise. In order to measure this interval, the toise being taken away, very fine lines were drawn with a fine point, at the end of each cheek, upon the brass pins which were in the same plain with the board: then the cheeks were removed, and fine points made at the outer extremity of each line, and this distance being taken between the fine points of a beam compass, was transferred to the scale, and thus the length of the toise was found in measures of the scale, which is divided by a vernier to thousandths of an inch. The toises and brass scale had been left together in the same room, and near one another all the night before, and till the very time of making the comparison of the toises with the scale, in order to be sure that they were all affected with the same degree of heat.

As it may be agreeable to the reader to see the result of the principal measures of degrees of latitude, that have been taken with later instruments and proper accuracy, brought together into one view, the following table is here added.

Length of a degree in Paris toises.	Mean latitude.			Names of the observers.	Years in which the degrees were measured.
57422	66°	20′	N	M. de Maupertuis, &c.	1736 and 1737
57074	49	23	N	M. de Maupertuis, &c. and M. Cassini	1739 and 1740
57091	47	40	N	P. Liesganig	1768
57028	45	0	N	M. Cassini	1739 and 1740
57069	44	44	N	P. Beccaria.	1768
56979	43	0	N	Le Pere Boscowich and Le Maire	1752
56888	39	12	N	Mess. Mason and Dixon	1764 to 1768
56750	0	0		M. Bouguer and M. de la Condamine	1736 to 1743
57037	33	18	S	Abbé de La Caille	1752

If this degree be compared with the degree measured at the equator = 56750 toises, in the hypothesis of the earth's being an oblate spheroid, the ratio of the equatorial to the polar diameter will come out as 494 to 493. But, if it be compared with the degree measured in Laplandy, in the latitude 66° 20′, = 57419 toises (I have subtracted 3 toises, because the toise used in Laplandy was [illegible]th or $\frac{1}{25}$th of a line less than the toise used in Peru, see M. De la Lande's Astronomy, Article 2107), the ratio of the diameters will be as 142 to 141. The great difference of these results is a fresh proof of what has appeared from the comparison of the measures of the several degrees taken before, either that the figures of the meridians are not accurately elliptical, or that the inequalities of the Earth's surface have a considerable effect in deflecting the plumb-line from its true situation, or both. I had indeed supposed that any deflections of the plumb-line were not to be feared with respect to this particular measure of a degree, at the end of my Introduction to Messieurs Mason and Dixon's account of the same, by arguing

arguing, perhaps too far, from the level disposition of the country through which the degree passes. But the Honourable Mr. Henry Cavendish has since considered this matter more minutely; and having mathematically investigated several rules for finding the attraction of the inequalities of the Earth, has, upon probable suppositions of the distance and height of the Allegany mountains from the degree measured, and the depth and declivity of the Atlantic ocean, computed what alteration might be produced in the length of the degree, from the attraction of the said hills, and the defect of attraction of the Atlantic; and finds the degree may have been diminished by 60 or 100 toises from these causes. He has also found, by similar calculations, that the degrees measured in Italy, and at the Cape of Good Hope, may be very sensibly affected by the attraction of hills, and defect of the attraction of the Mediterranean Sea and Indian Ocean.

The rules, which I used in calculating the ratio of the equatorial diameter to the polar axis, from the North American degree, compared with those measured in Peru and Lapland, are those given by Mr. John Robertson, Librarian to the Royal Society, in his Elements of Navigation, p. 597, as deduced by him from Dr. Letherland's Geometrical Analysis of the problem, which he has also given to the public in the same place, together with some other problems depending upon it, which were necessary to complete the subject.

XLIII. *Astronomical Observations, made in the Forks of the River* Brandiwine *in* Pennsylvania, *for determining the going of a Clock sent thither by the* Royal Society, *in order to find the Difference of Gravity between the Royal Observatory at* Greenwich, *and the Place where the Clock was set up in* Pennsylvania; *to which are added, an Observation of the End of an Eclipse of the Moon, and some Immersions of* Jupiter's *First Satellite observed at the same Place in* Pennsylvania: *By* Charles Mason *and* Jeremiah Dixon.

Read December 15, 1768.

The Place where these Observations were made is the Northernmost Point of the Lines that were measured for a Degree of Latitude, or Point N. (see TAB. XIII. fig. 2.) relative to that Measure; it lies 31 Miles West, by Measurement; and 10″,5 South of the Southernmost Point of the City of Philadelphia, as found by the Sector.

1766 Time per Clock.

Decemb.	d	h	′	″	h	′	″	
☿	24	4	28	40	. . . .			Equal Altitudes of Capella.
			30	18+	5	25	3	
. . * . .			32	$5\frac{1}{2}$		26	40	
	28	4	28	41	5	20	59	Altitudes of ditto.
. . * . .			30	22+		22	47−	
			32	10		24	$30\frac{1}{2}$	
	30	4	5	$52\frac{1}{2}$	5	43	24+	Equal Altitudes of ditto.
			7	12		44	$46\frac{1}{2}$	
. . * . .			8	34		46	$4\frac{1}{2}$	
1767								
January.								
	1	4	10	33	5	37	45	Equal Altitudes of ditto.
			11	55		39	10	
			13	$19\frac{1}{2}$		40	31−	

1767		Time per Clock.						
January.	d	h	′	″	h	′	″	
☿	7	3	50	26	5	55	8½	Equal altitudes of Capella.
			51	39+		56	27	
			52	56½		57	41½	
♃	8	4	4	48	5	40	3	Equal altitudes of ditto.
			6	7½		41	24	
			7	30—		42	44½	
. * . .		12	59	30				The first Satellite of Jupiter immerged. Apparent time 8d 17h 42′ 47½″
♄	10	6	21	28	8	2	45	Equal altitudes of Castor.
. . * . .			22	43		4	3	
			24	00		5	18—	
		7	34	18				The first Satellite of Jupiter immerged. Apparent time 10 12 10 23.
♀	16	4	5	4+	5	35	23	Equal altitudes of Capella.
. . * . .			6	25		36	47	
			7	50		38	7	
☽	19	4	4	8½	5	34	44	Equal altitudes of ditto.
. . * . .			5	27½		36	8	
			6	52		37	29	
♂	27	3	32	53	6	2	7	Equal altitudes of ditto.
			34	5—		3	21½	
			35	19½		4	34½	
February.								
♂	3	4	21	12½	6	36	10½	Equal altitudes of β Aurigæ . . Windy.
. . * . .			22	22		37	24	
			23	35+		38	34 :	
☿	4	3	34	52—	5	56	0½	Equal altitudes of Capella.
			36	5		57	16+	
			37	20		58	29	
☉	8	3	55	32	5	33	5½	Equal altitudes of ditto.
. . * . .			56	50+		34	26+	
			58	12		35	45½	
☽	16	13	44	50				The first Satellite of Jupiter was not immerged } flying clouds.
[illegible]			46	25				Ditto was immerged.
[illegible]	[illegible]	[illegible]	[illegible]	45—	5	7	9	Equal altitudes of Capella.
			[illegible]	[illegible]		8	55½	
			[illegible]	7—		10	32+	
[illegible]		12	58	[illegible]				First Satellite of ♃ immerged. Ap. time 25d 12h 24′ 40″ ::

From

From these observations we have the time of Capella's passing the meridian, and the rate of the clock's going as follows:

1766 Decemb.	✱ passed merid. per clock. h ′ ″	Clock loses of Sid. time per day. ″	Mean state of therm. °
24	4 57 40+	16,3	35
28	56 35	18,0	23
1767 30	55 59	13,4	6
January. 1	55 32+	14,8	37
7	54 3	17,0	20
8	53 46	16,3	37
16	51 36	16,0	31
19	50 48+	15,63	33
27	48 43+	15,35	28
February. 4	46 40½	15,5	30
8	45 38½	15,9	35
25	41 8−		

1764 March.	☽ eclipsed Time per watch. h ′ ″		
♄ 17	8 4 10	Eclipse of the ☽ ended.	
		h ′ ″	
	8 58 46	10 27 30::	Equal altitud. of Regulus.
	9 1 16	29 41	
	4 5	32 9	

The watch went very regular sider. time.

—Hence the eclipse ended at 8h 21′ 59″ app. time, in the forks of the river Brandiwine.

N. B. The edge of the earth's shadow on the ☽'s disk was the best defined I ever saw: it was remarkably distinct from the penumbral shade.

N. B. The clock was firmly screwed to a piece of timber, 22 inches in breadth, and five inches and a quarter thick; the said piece of timber was let four feet into the ground, which was composed of a very firm, dry, hard clay.

The clock was placed in a tent, with Fahrenheit's thermometer hung to its side; and a blanket was wraped round the clock and thermometer, to secure it from any wind that might enter the tent. The pendulum was adjusted to the upper scratch, with N° 3. at the Index, as directed by the Rev. Mr. Maskelyne, Astronomer Royal: but the spring at the suspension of the pendulum having been broke, (when the ship, in which it was sent, was wrecked on the Jersey coast) we cannot be certain that the pendulum is now of the same length as it was when sent from London.

Those observations marked : are a little dubious; those marked :: are very dubious; those marked . . ✱ were made per Mr. Dixon. The eclipses of ♃'s satellites were observed with a reflecting telescope of one foot focus, that magnified about 70 times.

1766 Decemb. d	Height of the ther. at about 7h in the mor. in the Tent	Air	Height of the ther. at about 2h in the after. in the Tent	Air	Vibration of the pend. on each side of O. that is, half the arch of vibration. ° ′	
☿ 24	..	..	43	45−		
25	..	..	44	46		
26	38	37	45	47	1 40−	
27	38	41	40	42		
28	21	18	31	26	1 35	Near midn. the ther. in the Tent 20, Air 16
29			28	28		At 10h¼ P. M. therm. in the Tent 29, Air 28
30			32	32		Near midn. in the Tent 17 Air 14
31	5 above O.	3 below O.	18	20	both above O.	

		Height of the ther. at about 7^h in the mor. in the Tent	Air	Height of the ther. at about 2^h in the aft. in the Tent	Air	Vibration of the pend. on each side of O. that is, half the arch of vibration.		
1766 Decemb. [d]								

31 At $8^{h}\frac{1}{4}$ P. M. ther. in the tent at O. in the air at 7 below O.

1767 At $10^{h}\frac{1}{4}$ P. M. ditto in the tent at 3 below O. in the air at 13 below O.

Jan. 1 At $7^h\ 6'$ in the morn. the ther. in the $\left\{\begin{matrix}\text{Tent} & 10\\ \text{Air} & 20\end{matrix}\right\}$ below O.

At $0^h\ 45'$ P. M. ther. in the $\left\{\begin{matrix}\text{Tent} & 21\\ \text{Air} & 17\end{matrix}\right\}$ above O. Vibration $= 1^\circ\ 12'$

At $11^h\ 4'$ P. M ditto in the $\left\{\begin{matrix}\text{Tent} = 3\\ \text{Air} = 12\end{matrix}\right\}$ below O Vibration = 1 10

---

2 At $6^h\ 42'$ in the morn. ther. in the $\left\{\begin{matrix}\text{Tent} & 9\\ \text{Air} & 22\end{matrix}\right\}$ below O.

At 10 ditto vibration of the pendulum $= 1^\circ\ 5'$ on each side of O. The pendulum now swings a little farther on the west side of O. than on the east side. The clock faces the north.

				21	15	1°	7′	At 9 P. M. In the {Tent 9, Air 5}
♄	3	11	9					
☉	4	before ☉ rise 34		39	39	1°	20′	
	5	37	37	48	49	1	35	Pend. now swings rather farthest on the east side of O.
	6	49		53	54	1	40	At $8\frac{1}{2}$ P. M. in the {Tent 43, Air 44}

---

7 At 11^h P. M. in the tent 25, in the air 26.

	8	28		at $10\frac{1}{2}$ P. M. ther. in the tent 23.				
	9	17		40	40	1°	35′	Pend. swings 8′ more on the east side of O. than on the w.
	10			43				At midn. ther. in the air 25.
☉	11			50	47			
	12							At 4 P. M. in the {Tent 42, Air 44}
	13			42	45	1°	40′	The pend. swings as on the 9th.
	14	23		33	33			
	15	33		39	41			

1767 January	d	Height of the ther. at about 7^h in the mor. in the Tent	Air	Height of the ther. at about 2^h in the aft. in the Tent	Air	Half the arch of vibration.	
	16	30	30	39	37	1° 35′	The pend. swings as before.
		At 9^h $5\frac{1}{2}$ P. M. ther. in the Tent 24 Air 21					
	17 at 9^h A. M.	28 tent	25 air	43	39		
☉	18	33	31	39−	39		
	19	25	26	39	36		At 9^h $\frac{1}{2}$ P. M. ther. in the Tent 21 Air 18
	20			39	40		
	21	39	39	40	40		
	22	23	21	27	27	1° 30′	The pendulum swings to the eastward as before.
	23	25	23	32	32		
	24	32	32	43	40	1° 30′	Wound up the clock.
	15	32	32	31	30		
	26	28	27	At 4^h $\frac{1}{2}$ P. M. ther. in the Tent 32 Air 32			
♂	27	21	20	At 4^h $\frac{1}{2}$ P. M. in the Tent 27 Air 25			
				9 ditto Tent 15 Air 12			
	28	11	14	36	32	1° 20′	The pendulum swings as before.
	29	15	13	35	34		
	30	16	16	31	35	1 20	
	31	32	35	At 4^h $\frac{1}{2}$ P. M. in the Tent 36 Air 36			
Feb.	1	36	35	36	37		
	2	15	13	40	34		
	3	16	15	41	38	1° 30′	
				At 9^h $\frac{1}{2}$ P. M. in the Tent 26 Air 25			
	4	14	10	34	32	1° 30′	
		At 9^h P. M. in the Tent 24 Air 23					
	5	30	32	45	41		
	6	13	12	28−	24	1 30	
	7	13	12	34	36		

1767 Febr.	d	Height of the ther. at about 7^h in the mor. in the Tent	Air	Height of the ther. at about 2^h in the aft. in the Tent	Air	Half the arch of vibration.	
	8	25	24	54	52	1° 35′	
		At $8^h\frac{1}{2}$ P. M. in the { Tent 33 — Air 32					
	9	32	32	42	41		
	10	41	41	34	35		
	11	25	25	40	38	1 40	The pendulum swings as before.
	12	30	29	38	41		
	13	31	31	32	33		
	14	28	24				
	15	26	27	At 4^h P. M. in the { Tent 34 Air 33			
	16	18	10	39	48		
	17	25	17	28	28		
	19			39	44		
♀	20 near noon { tent 46 air 55			48	59		
☉	22	14	12				
♄	28				69	1° 40′	{ Pend. vibrates about 8′ farther on the E. side of O, than on the W. side of O, as before.
March	1				56		
	2				46		
	3				57		
	4				49		The point of the pendulum swings something farther back from the arch (shewing the degrees and minutes) than it did when it was set up.
	5				51		
	6				51		
	7				48		
	8				56		
	9				51		Took down and packed up the clock.
	10				50		
	12	at ☉ rise 11			26		
	13	ditto 7			28		
	14				36		
	15				47		
	16				71		
	17				67		
	18	snow					
June	4				91		
	5				95		
	6				95		

1767 June	d	Height of the ther. at about 7^h in the mor. in the Tent	Air	Height of the ther. at about 2^h in the aft. in the Tent	Air	
	7				93	
	8				91	The air much altered, being very cool and pleasant.
	9				80	
	10	At $4^h \frac{1}{2}$ P.M.		90 at 7^h P.M.	80	

N. B. The thermometer is in the ſhade, and in the ſame place it was in laſt winter.

XLIV. *Extract of a Letter from* Rome, *to* M. Maty, *M.D. Sec. R. S. on the extraordinary Heats observed there this last Summer.*

Rome, August 27, 1768.

Read December 15, 1768.

AS I remember, when Mr. M— was here, he ſeemed deſirous to aſcertain the degrees of heat and cold; I cannot help mentioning the exceſſive heat of this ſummer, which is much greater than has been known in Rome for many years. Friday, the 19th inſtant, the mercury in a well-regulated thermometer according to Fahrenheit's ſcale, expoſed at a North window, where there was no ſun and very little reflection, ſtood from ten o'clock in the morning until about five in the evening at ninety-nine. About half an hour after ſunſet it fell to ninety, and at midnight was fallen to eighty-five, where it remained all night. This is the hotteſt day we have had, but for theſe three weeks paſt at midday the mercury has been always above ninety four, and at midnight ſeldom under eighty-three, which is the more extraordinary as I do not remember to have obſerved any other ſummer above eighty-nine at midday, nor above ſeventy-five at midnight. Notwithſtanding this great heat, there was never a more healthy ſummer at Rome; all the hoſpitals are almoſt empty.

James Byres.

XLV. *An*

Received November 21, 1768.

XLV. *An easy Method of making a Phosphorus, that will imbibe and emit Light, like the* Bolognian *Stone; with Experiments and Observations; by* John Canton, *M. A. and F. R. S.*

To make the Phosphorus.

Read December 22, 1768.

CALCINE some common oyster shells, by keeping them in a good coal fire for half an hour; let the purest part of the calx be pulverized, and sifted; mix with three parts of this powder one part of the flowers of sulphur; let this mixture be rammed into a crucible of about an inch and a half in depth, till it be almost full; and let it be placed in the middle of the fire, where it must be kept red hot for one hour at least, and then set by to cool: when cold, turn it out of the crucible, and cutting, or breaking it to pieces, scrape off, upon trial, the brightest parts; which, if good phosphorus, will be a white powder; and may be preserved by keeping it in a dry phial with a ground stopple.

The quantity of light a little of this phosphorus gives, when first brought into a dark room, after it has been exposed for a few seconds, on the outside of a window to the common light of the day, is sufficient to discover the time by a watch, if the eyes have

have been ſhut, or in the dark, for two or three minutes before.

By this phoſphorus celeſtial objects may be very well repreſented; as Saturn and his ring, the phaſes of the Moon, &c. if the figures of them, made of wood, be wetted with the white of an egg, and then covered with the phoſphorus. And theſe figures appear to be as ſtrongly illuminated in the night, by the flaſh from a near diſcharge of an electrified bottle, as by the light of the day.

Experiment I.

Having put ſome of the ſame parcel of the phoſphorus into two glaſs balls, and ſealed them hermetically; I placed one of them on the outſide of a window facing the South, that it might be very much expoſed to the direct rays of the Sun, where it remained from the 25th of December 1764, to the 25th of December 1765. The other was kept during the ſame time in darkneſs. After this, they were both expoſed to the light, and carried into a dark room together; where the phoſphorus in each appeared equally bright.

Experiment II.

Some of the phoſphorus finely powdered, being put into a glaſs ball, with as much water as would make it adhere to the glaſs, ſo as to cover the inſide of the ball, which was hermetically ſealed, was found gradually to loſe its property of imbibing a light, but faſter in ſummer than in winter; ſo that at

at the end of the firſt year, it could not, in the leaſt, be perceived to ſhine, when taken from the ſtrongeſt day-light, and carried into a dark room. It was alſo obſerved to loſe its whiteneſs by degrees, and to become of a very dark colour, eſpecially on that ſide of it next to the glaſs. Some of the phoſphorus which was made to ſtick to the inſide of a glaſs ball hermetically ſealed, by means of common ſpirit of wine, was found after one year to be a little impaired; but ſome made to ſtick by means of an ætherial ſpirit, was found not to be impaired at all.

According to Doctor Nicholas Lemery (in his Courſe of chymiſtry, eleventh edition) the expoſing the Bolognian ſtone to the Sun wears it out. But by the firſt experiment it appears, that a phoſphorus of the ſame kind was not hurt by the Sun in twelve months. Water, indeed, in the ſecond experiment, was found in that time to deſtroy it. Therefore it is probable, that what the Doctor imputed to the light of the Sun, was cauſed by the moiſture of the air.

Experiment III.

I mixed a ſmall quantity of the phoſphorus with a conſiderable quantity of ſpirit of wine in one glaſs ball, and with æther in another, and ſealed them hermetically. When the balls were ſhook, each of the fluids appeared like milk; but the phoſphorus would ſoon ſubſide when the balls were at reſt, and leave the ſpirit of wine and æther quite clear. After ſome months, the ſpirit of wine was found to be tinged with yellow; but the æther, to this time, remains unaltered. By ſhaking the balls while they are expoſed

to the light, the whole of the fluid in each, will appear luminous when carried into a dark room. The æther gives as much light now, as it did at firſt; but the ſpirit of wine a little leſs.

Experiment IV.

I expoſed the dry phoſphorus, in one of the glaſs balls mentioned in the firſt experiment, to the light of the day, by holding it on the outſide of a North window about half a minute; after which it was kept in darkneſs for two days and a half, and was then found to ſhine, by putting the glaſs ball that contained it into a baſon of boiling water. On the morrow it was expoſed to the light again; and after it had been kept four days and a half in the dark, it gave light when put into boiling water, though not ſo much as before. In ſummer, I find, it will not give any light by the heat of boiling water after keeping it fifteen days; but in winter, it will afford a little, after keeping it a month.

Experiment V.

The phoſphorus in each of the two glaſs balls mentioned in the firſt experiment was illuminated at the ſame time and to the ſame degree, and carried into a dark room. One of the balls was immediately put into a baſon of boiling water, and thereupon the phoſphorus in it became much brighter than that in the other, and continued ſo for a ſhort time, but parted with its light ſo faſt, that in leſs than ten minutes it was quite dark. The other phoſphorus ſtill gave a conſiderable

degree

degree of light, and remained visible for more than two hours after, when even the heat of the hand would plainly increase its light.

Bolognian phosphorus is said, by Lemery, and also by Musschenbroek *, to imbibe less light when hot than when cold, as it appears less bright when carried into a dark room. But this appearance may be caused by its parting with the light it has received faster when in the former state, than when in the latter, according to the last experiment; as it must lose more when hot, than when cold, during the time of conveying it from the place where it takes the light, to a place dark enough to observe it in. And this seems to be the cause also, why Bolognian phosphorus never appears so bright after it has been illuminated, and consequently in some measure heated, by the direct beams of the Sun, as after it has only been exposed, in the shaded open air, to the common light of the day.

Experiment VI.

The balls used in the last experiment were kept in the dark for two days after, and then each at the same time was put into a bason of boiling water in a dark room: that which had parted with its light in the hot water before, was not visible; but the other appeared luminous for a considerable time.

When the phosphorus has once lost as much of the light it had received, as the heat of boiling water will

* See his *Introductio ad Philosophiam Naturalem*, § 1697. See also § 1704 and 1686.

cause

caufe it to part with, it has never after been found, if kept in darknefs, to give any more light by that degree of heat. But if it be expofed again to the common light of the day, the experiments may be repeated with the fame fuccefs as before. This has frequently been done, with fome dry phofphorus in glafs balls which have been hermetically fealed about four years, without the leaft injury to the phofphorus; as it appears to be as good now as it was at firft.

Experiment VII.

Let one end of a bar of iron of about an inch fquare, or a poker, be made red hot, and laid horizontally in a darkened room, till by cooling it ceafes to fhine, or is but barely vifible. Then bring a little dry phofphorus, which has been expofed to light in a glafs ball hermetically fealed, as near the hot iron as poffible, by holding the ball in contact with it; and the phofphorus, though invifible before, will in a few feconds begin to fhine; and will difcharge its light fo very faft as to be entirely exhaufted of it in lefs than a minute; and then will fhine no more by the fame treatment, till after it has been expofed to light again. By this heat, light received from a candle, or even from the Moon, may be feen feveral days after. And phofphorus that will afford no more light by the heat of boiling water, will fhine again by the heat of the iron. By this heat alfo, phofphorus which had been kept in darknefs more than fix months, was found to give a confiderable degree of light.

It was the opinion of the great Sir Ifaac Newton, that the rays of light are very fmall bodies emitted from

from ſhining ſubſtances, and not motion propagated through a fluid medium; for ſeveral reaſons which he has given in his Opticks. Notwithſtanding which, it has been urged ſince his time, that light is nothing but a repellent fluid put into very violent vibrations. Now it appears impoſſible, to me, at leaſt, if light be nothing but motion propagated through a fluid medium, and not particles emitted from the luminous body, to account for the phænomena in the fifth, ſixth, and ſeventh experiments. That a ſubſtance ſhould either give light or not, when its parts are agitated by the ſame degree of heat, according as it has, or has not been expoſed to light, for a few ſeconds of time, more than ſix months before; ſeems plainly to indicate a ſtrong attraction between that ſubſtance and the particles of light; by which it keeps many of them, in the common heat of the air, a long time, if not always: for the light the phoſphorus gives by being heated to a certain degree appears to be cauſed by its throwing off adventitious particles, and not by any of its own; ſince its light will decreaſe and be entirely gone, before the phoſphorus will be hot enough to ſhine of itſelf, or to emit particles of light from its own body.

A writer againſt the Newtonian doctrine of light is preſſed with a great difficulty, and aſks, if it be poſſible that a particle can move ſo far as from the Sun to the Earth, and not frequently impinge upon other particles, when, he ſays, every part of ſpace muſt contain thouſands of them? But this difficulty will nearly vaniſh, if a very ſmall portion of time be allowed, between the emiſſion of every particle and the

the next following in the ſame direction. Suppoſe, for inſtance, a lucid point of the Sun's ſurface to emit 150 particles in one ſecond, which are more than ſufficient to give continual light to the eye, without the leaſt appearance of intermiſſion; and then the particles, on account of their great velocity, will be behind one another more than 1000 miles, and leave room enough for others to paſs, in all directions.

XLVI. *Astronomical Observations made at* Swetzingen, *in the Years* 1767 *and* 1768; *extracted from several Letters written to* Charles Morton, *M. D. Sec. R. S. and one to the late Earl of* Morton. *By Father* Christian Mayer, *F. R. S. Astronomer to the Elector Palatine.*

Illustrissimo ac celeberrimo Viro ac Domino D. Carolo Morton, Societatis Regiæ Londinensis Secretario, ac ejusdem Synedro, et Academiarum Petropolitanæ et Cæsareo-Leopoldinæ Socio, &c.

S. P. D.

Christianus Mayer S. J. S^mi Elect. Pal. Astron. ejusdemque Societatis Regiæ Lond. et Instit. Bononiensis Socius.

Read Dec. 22, 1768.

QUAM tibi mitto observationem eclipsis lunaris, die 3 Jan. hujus anni, in specula electorali Schwetzingensi a me factam, jam velim ita accipias, ut si qui sint defectus in ea commissi, eos non tam mihi quam tempori parum faventi tribuendos putes: paucas enim, quas vides, positiones micrometri, e nubibus fere piscari oportuit, vix semel luna nec nisi prope finem meliori luce refulgente; ob quam causam maculæ nullius immercerto notare potui. Nihilominus

ominus quantitatem obſcurationis, atque ejus initium ac finem bene a me obſervata cenſeo, neque cum hanc obſervationem a notis Gallorum et Italorum ephemeridibus tantum diſtare video, ubi potiſſimum hujus erroris fontem inquiram, num in latitudine lunæ an in ejus longitudine, ſatis apud me conſtitutum habeo: ſuſpicor tamen, eum a latitudine potiſſimum repetendum eſſe; ſiquidem ſupputatione ex iiſdem Gallorum tabulis a me facta ac diminuta 30″ latitudine lunæ, quam cl. De la Lande pro tempore oppoſitionis invenerat = 44′. 42″ quantitatem obſcurationis obſervationi omnino conformem reperio, non ita tempus veræ oppoſitionis, quod tum adhuc, mutatis varie elementis a calculo ejuſdem De la Lande uno alterove minuto diſſidere deprehendi. Obſervationi huic adjeci aliquot culminationes fixarum ac planetarum, ſolſtitium hybernum proxime circumſtantes, quas ſcio aſtronomis haud ingratas eſſe ob multiplicem uſum comparationum ad perfectionem tabularum tendentium, in quibus equidem occurrent nonnulla, quæ aſtronomis Anglis maximæ preciſioni aſſuetis minus ſatisfaciant; verum vel ex eo, illuſtriſſime Morton, facile ipſe intelliges, tecumque intelligent univerſi periti rerum arbitri, quid mihi deſit, quamque difficile ſit, uno quadrante Pariſino mobili $2\frac{1}{2}$ ped. in radio multo plura præſtare. Quis enim, cum per diem ejuſmodi quadrante utitur ad capiendas ſolis altitudines reſpondentes, ingruente veſpera idem inſtrumentum ita in plano meridiani ſemper collocet, ut non pluribus ſecundis ab eo ad ortum vel occaſum declinet? Quis culminatione unius ſideris labore altitudinum reſpondentium exacte determinata non in eandem difficultatem recidat; cum reliquorum ſiderum

derum aſcenſiones rectas a vero ſitu ejuſdem quadrantis pendere nihilominus experiatur? Equidem utor in eum finem meridiano filari ad debitam poſitionem plani quadrantis percommodo, ſed contingit non raro, ut creſcente frigore aut calore nulla alia cauſa ſaltem mihi manifeſto apparente, quas bene cœpi obſervationes, exigua deviatio fili penduli turbet, dubiaſque proin reddat altitudines ſiderum apparentes. Huic quoque malo promptam medelam afferre licet exigua elevatione aut depreſſione cochlearum quadrantem ſuſtinentium, verum non ſine manifeſto periculo deſtruendi planum verticale. Taceo reliqua inſtrumenti hujus vitia: hæ certe difficultates ſunt tantæ, ut iiſdem colluctari diutius aut non poſſim aut nolim, quibuſque nemo citius meliuſque liberare me poteſt quam Anglus artifex.

Datum Heidelbergæ, die
15 Jan. 1768.

Obſervationes cœleſtes factæ Schwetzingæ in Specula Arcis Electoralis, anno 1767 et 1768.

TABULA MERIDIEI VERI.

Dies anni 1767 et 1768.		Meridies verus et correctus.			Altitudo limbi ſolis apparenter ſuperioris necdum purgata à refractione et parallaxi.		
Dec.	22	12	16	51,5*	16	55	46
	23	12	17	23,6*	16	56	20
Jan.	1	12	24	7,0			
	2	12	24	46,5*			
	3	12	25	27,0			
	4	12	26	8,5			
	5	12	26	51,0			
	6	12	27	34,5*	17	51	35
	7	12	28	15,5			

Meridies aſteriſco * notati indicant tempus verum meridiei illius diei exacte per altitudines ſolis correſpondentes determinatos eſſe.

Notandum, die 24 Dec. horologium pendulum duobus circit. min. quietum constitisse; ob eamque causam meridiem diei 1 Jan. nequaquam consentire cum meridie dierum 22 et 23 Dec. habita nempe ratione ordinatæ accelerationis; a die vero 1 Jan. usque ad 6 tum incrementum diurnum accelerationis penduli continuo augeri fere in progressione naturali una cum gradu frigoris per illos dies increscente, ut adjecta Tabula docet.

TABULA.

	Maximus grad. frigoris in Therm. Reaumuriano.	Quantitas accelerationis penduli supra motum medium Solis. ′ ″
Jan. die 1	12,5 infra 0	0 11,3
2	13,0	12,5
3	15,5	14,1
4	16,0	15,5
5	18,5	16,9

Altitudines quædam fixarum tempore vero culminationis proxime observatæ Schwetzingæ Quadrante $2\frac{1}{2}$ ped.

1767.		Temp. verum. h ′ ″	Altitudines refractione non purgatæ. ° ′ ″	
Dec.	22	5 52 47,2	68 26 47	Caput Andromedæ, α 2.
		5 57 44,0	54 32 34	Extrema in ala Pegasi Algen γ 2.
		7 36 58,0	58 47 43	In aure Arietis, γ 4.
		11 56 19,5	63 11 34	Pes Castoris, η 4.
		11 57 37,0	63 5 35	♄ Saturnus in media filorum intersectione medius.
		12 4 20,0	63 14 35	In pede Pollucis, μ 4.
		12 19 41,6	57 13 0	Lucida in tibia Pollucis, γ 3.
		12 30 17,0	24 14 31	Syrius.
		12 45 37,5	61 31 40	In genu Castoris, ζ 3.
		13 1 29,2	63 1 45	In femore Pollucis, δ 3.
		17 50 52,2	17 14 0	In rostro Corvi, α 4.
		17 58 14,5	21 23 59	In ala præcedente Corvi, γ 3.
		18 14 25,3	25 44 22	Sidus 5tæ magnitud. intense rubrum ob limbum satis bene terminatum cometæ simile.
		19 7 7,2	30 42 15	Spica Virginis.
		19 8 59,2	34 4 55	Limbus apparenter superior ♃ Jovis.

Continuatio

Continuatio præcedentium Obſervationum.

		h	′	″	°	′	″	
Dec.	23	12	17	23,6				Meridies verus.
		5	52	56,9	54	32	43	Algen, γ 2
		7	32	21,0	58	47	20	In aure Arietis, γ 4.
		11	52	33,5	63	5	48	Centrum Saturni ♄.
		11	59	40,0	63	14	55	In pede Pollucis, μ 3.
		12	15	3,0	57	12	55	Lucida in tibia Pollucis, γ 3.
		13	30	18,0	16	22	40	In ſummitate Argonavis, ζ 4. Comitibus duabus ſtellulis.
		13	47	56,0	17	1	15	In puppi Argonavis, ρ 3.
1768.								
Jan.	2	11	4	38,5	63	6	55	Centrum Saturni.
		11	7	14,5	63	11	22	Pes Caſtoris.
		11	41	5,6	24	14	78	Syrius.

Culminationes Centri Lunæ Schwetzingæ obſervatæ, 1768.

			″
Mora Tranſitus Lunæ per merid. die	2 Jan.	2	26
	3 Jan.	2	24,7

					Altitudo limbi apparent. ſuperior.			
Jan.	2	10	52	21,1	65	37	0	
	3	11	48	42,6	64	0	10	
	6	14	16	27,2	52	8	20	
Eodem		20	40	13,8	24	10	3	Centrum Veneris ♀.

Hæ altitudines eo ſenſu apparentes ſunt, quod ab errore inſtrumenti, ut cæterorum ſiderum omnium altitudines, correctæ ſint, non item a refractione et parallaxi, aberratione, &c. Addenda quoque eſt ſemidiameter lunæ pro habenda vera altitudine centri.

Barometrum die 22 Dec. in meridie fuit 27 dig. 11$\frac{1}{2}$ lin. veſperi eodem die 28 dig. Die 23 = 28 dig. 1 lin.

Die 2 Januarii = 28 dig. 2 lin.
28 — 2$\frac{1}{4}$
27 — 11

Therm. Reaumurianum die 22 Dec. mane fuit 2 infra o, veſperi 4. Die 23 mane 6. veſperi 7. circa meridiem 5. infra terminum glaciei: de reliquis diebus conſtat ex ſuperiore Tabula.

Notandum,

7

Notandum, culminationes diei 22 Decemb. supra recensitas propiores vero esse, quam illas diei 23: siquidem facto calculo inveni caput Andromedæ α 2, die 22 Dec. culminare debuisse hora 5, 53′ 13″ temp. ver. id est 26″ tardius quam ex observatione habui 5^h 52′ 47″: veram autem culminationem ipsius *Algen* die 23 Dec. accidisse 5^h 53′ 39″ a T *v.* qualem observaveram 5^h 52′ 56″,9. Unde patet aberrationem plani quadrantis ad ortum pro die 22 Dec. fuisse circit. 26 secundorum; pro die vero 23 Dec. eam aberrationem fere duplo fuisse majorem ad ortum: dixi fere; quia hæc ipsa tempora vera culminationum, ut sint exactissima, aberrationis et nutationis correctiunculis emendanda forent.

Observationes duarum Immersionum primi Satellitis Jovis, factæ Schwetzingæ, tubo Dollondi, a Christiano Mayer, S. J.

	Temp. ver.		
	h		
Anno 1767, die 30 Dec. Satelles I. Jovis videri desinit	16	58	59
Anno 1768, die 6 Jan. Satelles I. adhuc apparet	18	48	51
Disparuit	18	49	4

Observatio posterior melior est prima, ob cœlum minus vaporosum.

Oppositio Lunæ ecliptica die 3tio Januarii, in Specula Electorali Schwetzingensi observata, a P. Christiano Mayer.

Tubo 6 pedum micrometrum habente, tempore nubilo.

Temp. ver. h	′	″		Partes Lunæ obscuratæ.	Digiti obscurati.	′
15	44	29	Penumbra incipit ad partes Lybiæ et Arab.			
	46	9	Eadem fit densior			
	48	55	Videtur eclipsis esse initium			
16	8	30	— — — — — —	686	2	40
	15	52	— — — — —	990	3	51
	20	54	— — — —	1334	5	11
	32	6	— — — —	1400	5	26
	36	6	— — — —	1412	5	28
	47	48	Maxima obscuratio observata — —	1458	5	40,9
			EMERSIONES.			
17	30	35	— — — —	1255	4	53

Temp

Temp. ver. h	′	″		Partes Lunæ obſcur.	Dig. obſ.	′
17	33	32	- - -	1166	4	32
	37	10	- - - - - -	976	3	48
	41	24	- - - -	807	3	9
	47	34	- - - -	726	2	50
	50	19	- - - -	672	2	38
	51	41	- - - -	576	2	16
	57	52	- - - -	474	1	52,3
18	1	38	- - - - - -	299	1	11,6
18	4	44	Finis dubius			
	6	20	Finis certus tubo 6 pedum			
	6	53	Idem finis tubo Dollondi ab alio obſervatus			

Diameter lunæ toto tempore obſervationis aſſumpta in partibus circuli ° 0 ′ 31 ″ 38
in partibus micrometri 3079 particularum.

Porro ex hac obſervatione patet durationem totius eclipſis fuiſſe $2^h\ 17'\ 25''$ eamque optime conſentire tabulis Gallicis et Italicis; medium autem eclipſis ex obſervatione inventum fuiſſe $= 4^h\ 57'\ 40''$, quod ex tabulis De la Caille debebat apud nos eſſe $5^h\ 3'\ 21''$; ex calculo De la Lande $5^h\ 1'\ 1''$; ex ephemeridibus Bononienſ. $5^h\ 2'\ 28''$; quantitatem obſcurationis a me ex obſervatione inventam 5 dig. 41′, quæ ab aſtronomis Pariſinis ſupputata eſt 4 dig. 52′ a Bononienſibus 4 dig. 53′.

Immerſiones Satellit. Jovis Swetzingæ obſervatæ, Menſi Martio, 1768.

			Temp. ver. h	′	″
Martii	8	Satelles I. ♃ obſcure adhuc apparet - - -	17	17	0
		Idem omnino diſparet - - - - -	17	17	18
	9	Satelles II. obſcurius adhuc apparet - - -	14	16	17
		Idem omnino diſparet - - - - - -	14	16	37
	9	Satelles III. obſcurius adhuc apparet - -	14	21	18
		Idem omnino diſparet - - - - - -	14	22	6
	10	Satelles I. adhuc obſcure apparet - - -	11	46	55
		dubie apparet - - - - - -	11	47	0
		omnino diſparet - - - - - -	11	47	15

		Temp. ver. h	′	″
Martii 24	Satelles I. luce imminuta limbum ♃is tangere videtur - - - - - - -	15	38	25
	Idem omnino disparet - - - - - -	15	38	34
26	Satelles I. obscurius adhuc apparet - -	10	7	35
	omnino disparet - - - - - -	10	7	45
27	Satelles II. obscurius adhuc apparet - - -	8	43	45
	Idem omnino disparet - - - - - -	8	44	6
Aprilis 11	Satelles I. prope limbum Australem Jovis emersisse videtur - - - - - -	10	41	26

Omnes hæ observationes noto tibi tubo Dollondi Schwetzingæ factæ sunt, cœlo præter morem puro, statu penduli et pridie et ipso plerumque observationis die per altitudines solis respondentes penitus explorato, atque inde deducto tempore medio ob variationem declinationis solis correctæ, fasciis Jovis semper apparentibus, et, ut æstimare poteram, sub angulo sex vel septem graduum ad axem majorem Jovis inclinatis ab ortu in occasum, alter enim axis minor fere directionem verticalis a Borea ad Austrum plus minus pro varia Jovis altitudine constanter sequi videtur, quam quidem compressionem figuræ Jovialis evidenter observavi, nunquam tamen melius, quam die 26 Martii, quo prope marginem Jovis australem paulo ante immersionem, maculam præterea rotundam situ inter ortum et occasum fere mediam in parte, ut dixi, disci Jovialis australi clare vidi.

Conferendo

Conferendo Immersiones supra dictas cum ephemeridibus De la Lande maxima differentia oritur, ex illa 10 Martii: nempe,

	′	″
	25	12
Minima differentia ex 8 Mart.	24	12
Differentia est	1	
Semi-differentia est	0	30
Differentia meridianorum	24	42
Sed habita ratione immersionis diei 11 April. differentia nostrorum meridianorum Parisios inter et Schwetzingam ex facta cum ephemeridibus collatione oritur	26	30
Differentia immersionum et emersionum	1	48
Cujus dimidium	0	54
Additum priori determinationi	24	42
Producit differentiam meridianorum	25	36

Illustrissimo et excellentissimo Domino, Domino Comiti de Morton, Societatis Regiæ Londinensis Præsidi dignissimo, &c. &c.

S. P. D.

Christianus Mayer S. J. Societatis Regiæ Londinensis et Instit. Bonon. Socius.

Heidelb. Jul. 20, 1768.

SUBJUNGO quod ex nupera eclipsi totali lunæ diei 29 Junii mihi inclementia cœli fecit reliquum. Cœlo omni spissis nubibus constanter obducto, luna non nisi paulo ante totalem sui immersionem horizonti proxima apparuit, quo tempore tres sequentes positiones tubo dioptrico sexpedili micrometro armato capere potui.

Anno 1768, die 29 Junii.

Tempus verum.			Partes lucidæ lunæ.			Partes micrometri.
h	′	″		′	″	
15	31	46	0	7	25,7	
15	34	51	0	5	53,4	575
15	39	34		3	31,5	345

	h	′	″
Ex his assumpta diametro horizontali lunæ 31′ 4″, et motu horario lunæ a sole 30′ 11″, inveni initium hujus eclipsis ad meridianum Schwetzingensem proxime accidisse	14	39	0
Totalem immersionem lunæ	15	51	21

	h	′	″	
Et reductione facta ad meridianum Londinensem speculæ domesticæ cl. D. Short	14	3	33	pro initio
Et pro immersione totali	15	15	44	

Quod si aliqua ratione observationibus illinc factis consentiat in tanta aëris nostri malignitate, fortunæ tribuo.

Emersiones aliquot Satellitum Jovis observatæ Schwetzingæ tubo Dollondi 10 ped. anno 1768.

	Temp. ver.		
	h	′	″
Die 27 Maii cœlo sereno			
Satelles III. omnino mihi ex oculis evanescit seu immergitur.	10	27	26
Eadem nocte Satelles I. observante ser^mo principe Wilhelmo de Gelnhausen, emergere videtur	11	9	44
Die 3 Junii emersio I^mi Satellitis a me observata inter nubes	13	5	5
Die 5 Julii emersio I^mi Satellitis cœlo sereno ab alio, me absente, observata	9	33	42

Hæc ultima observatio ab homine juvene exercitato facta, cœlo sereno, longe præferenda est illi die 3 Junii a me factæ, Jove fere inter atras nubes per vices incedente.

Christianus Mayer, S. J.

XLVII. Obser-

XLVII. *Obſervations of the Tranſit of Venus over the Sun, and the Eclipſe of the Sun, on* June 3, 1769; *made at the Royal Obſervatory. By the Rev.* Nevil Maſkelyne, *B. D. F. R. S. and Aſtronomer Royal.*

Read June 15, 1769.

THE weather, which had been cloudy or rainy here, with a ſouth wind, for the greateſt part of the day, began to clear up at 4 o'clock in the afternoon, the wind having returned to the weſt, the ſame quarter in which it had been the afternoon before, which was remarkably fine and ſerene, though it changed early in the morning preceding the tranſit. Towards the approach of Venus's ingreſs on the Sun, the ſky was become again very ſerene, and ſo continued all the evening, which afforded as favourable an obſervation of the tranſit here as could well be expected, conſidering that the Sun was only 7° 3′ high at the external, and 4° 33′ at the internal contact. I obſerved the external contact of Venus at $7^h\ 10'\ 58''$ apparent time, with an uncertainty ſeemingly not exceeding 5″; and the internal contact, by which I mean the completion of the thread of light between the circumferences of the Sun and Venus, at $7^h\ 29'\ 23''$ apparent time, with a ſeeming uncertainty of only 3″; for ſo long was the thread of light in forming, or the Sun's light in flowing round and filling up that part of his circumference which was obſcured by Venus's

Venus's exterior limb. Neverthelefs, I would not hence infer, that obfervations made by aftronomers in diftant places fhould agree together within fuch narrow limits; for I know they will not even in the fame place, and that a difference in the fkill or judgment of the obfervers, in the telefcopes, and perhaps in fome other little circumftances, not eafily diftinguifhed, may produce much greater difagreements, efpecially if the Sun be low, as it was here; in like manner as in obferving the eclipfes of Jupiter's fatellites, the immerfion or emerfion fhall often feem inftantaneous, or nearly fo, equally to two obfervers in diftant places, and yet the abfolute times of the obfervations may differ a minute of time or more from each other, owing to the difference of telefcopes, weather, or other circumftances. Indeed, in the prefent cafe, the limit of differences is certainly much narrower; but what it is I fhall not at prefent venture to fuggeft, as that may better be done when all the obfervations that fhall have been made of the tranfit are collected together. The telefcope which I ufed, was an excellent reflecting one of two feet focus, made by the late ingenious Mr. Short, and is the fame with which the laft tranfit was obferved here by Mr. Charles Green. I applied the magnifying power of 140 times, and ufed fmoaked glaffes to defend the fight, which are much preferable to black or red glaffes, as fhewing the objects more diftinct, and being much more pleafant to the eye.

I fhall now endeavour to defcribe, as accurately as I can, fome other phænomena which I noted during the immerfion of Venus, and to mention fome others, which by fome ingenious perfons were expected to have

have been ſeen, but which I could not diſcover.

It had been thought by ſome, that Venus's circumference might probably be ſeen, in part at leaſt, before ſhe entered at all upon the Sun, by means of the illumination of her atmoſphere by the Sun; I therefore looked out diligently for ſuch an appearance, but could ſee no ſuch thing.

I was alſo attentive to ſee if any penumbra or duſky ſhade preceded Venus's firſt impreſſion on the Sun at the external contact, ſuch a phænomenon having been obſerved by the Rev. Mr. Hirſt, F. R. S. at the former tranſit of Venus, in 1761, which he obſerved with much care and diligence at Madraſs, in the Eaſt-Indies; but I could not diſcern the leaſt appearance of that kind. I would not, however, be therefore thought to call in queſtion either Mr. Hirſt's diſcernment or fidelity; as I am ſenſible that the tremors of the limbs of the Sun and Venus, occaſioned by the vapours at the altitude of 7°, might eaſily obſcure a faint object.

When Venus was a little more than half immerged into the Sun's diſc, I ſaw her whole circumference completed, by means of a vivid, but narrow and ill-defined border of light, which illuminated that part of her circumference which was off the Sun, and would otherwiſe have been inviſible. This I might, probably, have ſeen ſooner, if I had attended to it. I continued to ſee it till within a few minutes of the internal contact, and grew apprehenſive that it would prevent the appearance of the thread of light, when it came to be formed; but it diſappeared about two or three minutes before, as well

well as I can remember: after which the regularity of Venus's circular figure was difturbed towards the place where the internal contact fhould happen, by the addition of a protuberance, dark like Venus, and projecting outwards, which occupied a fpace upon the Sun's circumference, which bore a confiderable proportion to the diameter of Venus. Fifty-two feconds before the thread of light was formed, Venus's regular circumference, fuppofed to be continued as it would have been without the protuberance, feemed to be in contact with the Sun's circumference fuppofed alfo completed. Accordingly, from this time, Venus's regular circumference, fuppofed defined in the manner juft defcribed, appeared wholly within the Sun's circumference; and it feemed, therefore, wonderful that the thread of light fhould be fo long before it appeared, the protuberance appearing in its ftead.

At length, when a confiderable part of the Sun's circumference, equal to $\frac{1}{3}$d or $\frac{1}{4}$th of the diameter of Venus, remained ftill obfcured by the protuberance, a fine ftream of light flowed gently round it from each fide, and completed the fame in the fpace of three feconds of time, from $7^h\ 29'\ 20''$ to $7^h\ 29'\ 23''$ apparent time; and Venus appeared wholly within the Sun's lucid circumference; but the protuberance, tho' diminifhed, was not taken away till about 20″ more, when, after being gradually reduced, it difappeared, and Venus's circular figure was reftored.

An ingenious gentleman of my acquaintance having defired me to examine if there was any protuberance of the Sun's circumference about the point of the internal contact, as he fuppofed fuch an ap-

pearance ought to arise from the refraction of the Sun's rays through Venus's atmosphere, if she had one; I carefully looked out for such a circumstance, but could see no such thing; neither could I see any ring of light round Venus, a little after she was got wholly within the Sun: but, I confess, I did not re-examine this latter point afterwards, when she was further advanced upon the Sun, at which time other persons at the observatory saw such an appearance.

How far the ring of light, which I saw round that part of Venus's circumference which was off the Sun, during the immersion, may deserve to be considered as an indication of an atmosphere about Venus, I shall not at present inquire; but I think it very probable, that the protuberance, which disturbed Venus's circular figure at the internal contact, was owing to the enlargement of the diameter of the Sun, and the contraction of that of Venus, produced by the irregular refraction of the rays of light through our atmosphere, and the consequent undulation of the limbs of the two planets; the altitude of Venus being only 4° 48′, though the Sun's limb was more distinct and steady than usual at that altitude. This conjecture seems corroborated by two circumstances: one is, that Venus's limb, from its first appearance to the total immersion, as well as afterwards, was very ill defined, and undulated very much; the other is, that her horizontal diameter, which I measured soon after the internal contact with an excellent achromatic object-glass micrometer, fitted to the two-feet reflecting telescope, was only $55\frac{3}{4}''$, by a mean of eight trials, or about 3″ less than it should have been, from the observations made, with the like instrument, at the

transit

transit of Venus in 1761, by Mr. Short, Mr. Canton, Mr. Haydon, and Mr. Mason, when the Sun was at a considerable altitude; and most likely the Sun's diameter was enlarged in proportion, though it might have been difficult to have ascertained it by actual measure, had time allowed me to make the experiment with the same micrometer before the Sun entered into a black cloud near the horizon.

Six other persons also observed the contacts of Venus here, and noted some other phænomena. Their names are, the Rev. Malachy Hitchins, a gentleman well acquainted with astronomy and astronomical calculations, who has made and examined many belonging to the Nautical Almanac, and has been so obliging as to come here and assist me in making astronomical observations, during the absence of my assistant, Mr. William Bayley, who is gone to the North Cape, by appointment of the Royal Society, to observe the transit of Venus there. The others are, the Rev. William Hirst, who observed the former transit of Venus, in 1761, at Madrass; John Horsley, Esq; a gentleman whom I had the pleasure of first commencing an acquaintance with during my voyage from St. Helena to England, in the Warwick East-India ship, and who then, and in several voyages since to the East-Indies and home again, observed and calculated the longitude from distances of the Moon from the Sun and fixed stars with the greatest accuracy; Mr. Samuel Dunn, who has had a good deal of practice in making astronomical observations, and who carefully observed the former transit of Venus, in 1761, at Chelsea; Mr. Peter Dollond, whose great skill in constructing achromatic and reflecting telescopes;

lescopes; and Mr. Edward Nairne, whose skill likewise in the same way, and in making all kinds of mathematical and philosophical instruments are sufficiently known to the public.

Mr. Horsley and Mr. Dunn observed with me in the great room; Mr. Hitchins and Mr. Hirst in the eastern summer-house; and Mr. Dollond and Mr. Nairne in the western summer-house; by three clocks placed in the respective rooms, which were compared with the clock in the transit room, before the external contact, and again after the internal contact was past; whence the times of the observations, as noted by the clocks, were reduced to the time of the transit clock, and thence to apparent time.

Their observations, together with my own, are given in the following table, as reduced to apparent time.

	External contact.			Regular circumferences in contact.			Thread of light compleated, or the internal contact.			Telescope made use of.	Magnifying power.
	h	′	″	h	′	″	h	′	″		
N. Maskelyne	7	10	58	7	28	31	7	29	23	2 feet reflector.	140
M. Hitchins	7	10	54	7	28	47	7	28	57	6 f. reflector.	90
W. Hirst	7	11	11	–	—	—	7	29	18	2 f. reflector.	55
J. Horsley	7	10	44	7	28	15	7	29	28	10 f. achromatic.	50
S. Dunn	7	10	37	7	29	28	7	29	48	3½ f. achromatic.	140
P. Dollond	7	11	19	–	—	—	7	29	20	3½ f. achromatic.	150
E. Nairne	7	11	30	–	—	—	7	29	20	2 f. reflector.	120

Mr. Dollond and Mr. Nairne used telescopes of their own construction; but they did not wait till the thread

thread of light was formed at the internal contact, but noted the time, when they judged it was just ready to be formed. The $3\frac{1}{2}$ feet achromatic telescopes were those made with 3 object-glasses.

The differences between the different observations seem pretty considerable, and greater than I expected, considering that all the telescopes may be reckoned pretty nearly equal, excepting the 6 feet reflector, which is much superior to them all; and to its greater excellence and distinctness I principally attribute the difference of 26″ by which Mr. Hitchins saw the internal contact before me; as I can depend upon his observations. Possibly the greatness of the differences might arise from the low altitude of the Sun and Venus; and then the like differences would not be so much to be feared in places where the observation may be made at higher altitudes; otherwise the sun's parallax will not be deducible from the transit of Venus with that accuracy which has been expected.

The other appearances about Venus, noted by the six observers, which they have communicated to me, are as follows:

Mr. Hitchins remarks, that, at the first contact, though there was a tremulous motion in the Sun's limb, yet that part of it which the planet entered was very well defined, and the first impression of Venus appeared to be instantaneous, and as a black, sharp point. At the internal coincidence of circumferences, the fluctuation of the Sun's limb was increased, and the limb of Venus being affected in like manner, there was an uncertainty of about 10″ in estimating the said coincidence; but at the breaking in of the thread of light between the limbs, there was not

not a greater uncertainty than a ſecond and half of time. At the internal coincidence of circumferences, the limb of Venus next to that of the Sun being protuberant, her vertical diameter appeared to be longer than the horizontal one; but when the Sun approached the horizon, and was ſcarce above a degree high, Venus's horizontal diameter appeared to be ſenſibly longer than the vertical, which was, probably, owing to refraction. After the internal contact, there appeared a luminous Ring round the body of Venus, about the thickneſs of half her ſemi-diameter; it was brighteſt towards Venus's body, and gradually diminiſhed in ſplendor at greater diſtances, but the whole was exceſſive white and faint. This radiancy round the planet ſeemed to him to be greater in Mr. Nairne's 2 feet teleſcope than in the 6 feet Newtonian reflector.

After the ſecond or internal contact, Mr. Hirſt left off obſerving with Mr. Dunn's 2 feet reflector, and had a ſight of Venus in the 6 feet Newtonian reflector, in which he thought he perceived a glimmering of light about the upper part of the circumference of Venus, or that part of the planet which entered laſt into the Solar diſc.

After Venus was got within the Sun's diſc, a light a little weaker than that of the Sun, of a purpliſh colour, appeared to Mr. Horſley, to the left-hand of Venus, which is really to the right, the teleſcope inverting objects. This light he ſaw for ſix or ſeven minutes.

From $7^h\ 28'\ 26''$ to $7^h\ 28'\ 30''$ apparent time, Mr. Dunn ſaw a very faint rim of light at Venus's

 exterior

exterior limb. After Venus was wholly on the Sun, he ſaw a faint ring of light ſurrounding her, both with the $3\frac{1}{2}$ feet teleſcope, and Mr. Nairne's 2 feet reflector.

When $\frac{1}{3}$d of Venus's diameter was entered upon the Sun, Mr. Dollond firſt ſaw a light about the exterior limb of the planet: this light, during all the time of its continuance, appeared rather reddiſh, and in all reſpects like irregular refracted light. After Venus was wholly entered upon the Sun, he ſaw a faint ring ſurrounding her.

After Venus was wholly entered upon the Sun, and her exterior limb was near one of her ſemi-diameters diſtant from the Sun's circumference, Mr. Nairne ſaw a faint light round the planet, rather brighter and whiter than the body of the Sun.

Fortunately, the weather was as favourable for the obſervation of the eclipſe of the Sun, the next morning, as it had been the evening before for that of the ingreſs of Venus upon the Sun; which is of the more conſequence, as the compariſon of it with the obſervations which may be made of it in the northern and eaſtern parts of the world will ſerve to ſettle the longitudes of thoſe places, and conſequently render the obſervations which may be made there of the tranſit more uſeful and valuable.

I obſerved the beginning of the eclipſe at $18^h\ 38'\ 54''$, and the end at $20^h\ 23'\ 30''$ apparent time, with the 2 feet reflector, uſing the magnifying power 90 times. And at $19^h\ 29'\ 31''$ apparent time, I obſerved the greateſt eclipſe, at which time I found the remaining lucid parts of the Sun $15'\ 15''$ with Dollond's micrometer, aſſuming the horizontal diameter of the Sun

31′ 31″, whence the value of the ſcale of the micrometer was determined for the preſent purpoſe. Hence the eclipſed parts of the Sun were 16′ 16″ or 6^{dig}. 11′,62 on the northern part of his diſc.

Mr. Hitchins obſerved the beginning of the eclipſe with a $3\frac{1}{2}$ feet achromatic teleſcope magnifying 150 times (the ſame with which Mr. Dollond obſerved the contacts of Venus), at 18^h 38′ 59″, and the end of the eclipſe with the 6 feet reflector with the magnifying power 90, at 20^h 23′ 35″ apparent time. And Mr. Samuel Dunn obſerved the beginning of the eclipſe at 18^h 39′ 9″, and the end at 20^h 23′ 33″ with the other $3\frac{1}{2}$ feet achromatic teleſcope, magnifying 140 times, the ſame with which he obſerved the contacts of Venus. Several inequalities in the Moon's circumference, ſeen upon the Sun's diſc during the eclipſe, were diſtinctly diſcerned by all of us, the air being very clear, and the objects ſteady.

The whole ſeries of meaſures of the lucid parts, which I took with the achromatic object glaſs micrometer applied to the 2 feet teleſcope, was as follows;

Apparent time.			Lucid parts.	
h	′	″	′	″
19	22	13	15	40,5
	24	21	15	26,5
	26	9	15	20,9
	28	26	15	15,6
	30	14	15	14,5
	31	44	15	16,4
	32	30	15	16,4
	33	19	15	19,8
	34	28	15	25,4
	36	19	15	35,9
	37	56	15	49,1

A N

E R R A T A.

P. 7. l. 10. *for* confusion *read* concussion.
P. 28. l. 8. *for* $Q^3 \times Q^2$ *read* $q^3 \times Q^2$.
P. 29. *in the note, for* 00379755 *read* 90379755.
P. 30. l. 2. *for* hypothesis *read* hypotheses.
Ibid. *throughout the table, for* ♂ *read* ♁.
P. 32. l. 16. *for* 1759 *read* 1761.
P. 175. l. 15. *for* being any number *read* z being any number.
P. 176. l. 4. *for* tohat *read* to that.
P. 179. l. 18. *for* $\frac{B}{\frac{1}{3}}$ *read* $\frac{B}{2^{\frac{1}{3}}}$
P. 203. l. 24. *for* thing thing *read* thing.

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